

# Three Essays on the Health Effects of Family Policies

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The Faculty of Business, Economics and Informatics of the University of Zurich hereby authorizes the printing of this dissertation, without indicating an opinion of the views expressed in the work.

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# Dissertation Overview

My thesis contributes to a growing literature in health economics by analyzing three empirical research questions related to family policies: What is the short- to long-run effect of parental leave duration on maternal health? Is there an impact of maternal labor force participation during pregnancy on newborn health? How do birth allowances affect fertility, newborn to infant health, and birth scheduling? In this introduction, I briefly summarize the chapters' research questions and main findings. All three chapters have in common that they answer highly topical questions in the domain of family policies, i.e. parental leave policies and family allowances. Further, to properly answer the stated questions, I make use of two key ingredients: First, I use rich administrative data from Austria for Chapters 1 and 2, and from Switzerland for Chapter 3. Second, I employ adequate and modern empirical techniques to report causal estimates.

For both Chapters 1 and 2, I exploit variation from three policy changes in parental leave duration in Austria. Specifically, Austria increased parental leave by 1 year to 2 years in July 1990. They partially reversed this again in July 1996 by exclusively reserving 6 months to fathers so that maternal leave was essentially reduced to 1.5 years. Finally in July 2000, there was a large extension in paid parental leave by 1 year to 2.5 years. Enforcement of all of these changes was very strict, meaning that mothers who gave birth in June prior to any of the policy changes would be entitled to the old leave duration and July mothers to the new one. This sharp discontinuity allows me to

employ a Regression Discontinuity Design (RDD) in Chapters 1 and 2. This method guarantees that mothers pre- and post-policy change are comparable in both observable and unobservable characteristics in absence of the policy change. This highlights that the only difference of these mothers across the cutoff is the result of the policy change itself.

For both Chapters, I rely on administrative data from Austria where I combine several different data sets. To identify private-sector employed mothers who were subject to the policy changes, I use the Austrian Social Security Database (ASSD), which covers the complete working history of every worker in Austria. With the ASSD, I have detailed information on every birth of employed mothers and their actual parental leave duration these mothers take. I can link the ASSD to the Austrian Birth Register (ABR) covering newborn health outcomes and additional individual-level characteristics of the mother, such as her education, marital status, and origin. In Chapter 1, where I study maternal health up to the long-run, I additionally merge the data to health outcomes recorded in the Health Insurance Data from the Statutory Health Insurance Fund (SHIF) of Upper Austria. This final data set records every outpatient doctor visit, prescribed medication, and hospital stays including diagnosis from 1998 to 2007.

I analyze the short- to long-run health effects of maternal leave duration in Chapter 1 and I find a hump-shaped relationship between parental leave length and maternal health. The first two reforms improve maternal outpatient health. However, health outcomes are getting substantially worse with long leave of 2.5 years. This effect is mainly driven by mental health outcomes. However, there is substantial heterogeneity. The initial increase in leave length is especially good for low-wage and unmarried mothers. Reducing leave duration harms mothers with unhealthy babies, proxied by a preterm birth or low birth weight baby. Substantially increasing leave duration is, though, especially bad for maternal health of those mothers who already suffered from mental diseases pre-birth.

Chapter 2 studies the effect of maternal employment during pregnancy on newborn health. Based on the three reforms on parental leave duration, I find that maternal employment during pregnancy with the second child reacts strongly to these policy changes. The share of employed mothers sharply declined in 1990 by 19.1 percentage points, increased in 1996 by 7.2 percentage points and declined again by 6.4 percentage points in 2000. None of these changes in prenatal employment translated into effects on newborn health measured via birth weight, gestational length, and Apgar scores. This result holds true for mothers of different socioeconomic backgrounds and across industries. The effect is precisely estimated, which suggests that prenatal employment prior to the 32nd week of pregnancy does not causally affect the fetus for measures visible at birth.

In Chapter 3, written jointly with Patrick Keller, we analyze the impact of birth allowances (so called baby bonus) on fertility, newborn health, and birth scheduling in Switzerland. We exploit the unique quasi-experimental setting of Switzerland's family allowances system. In this system, cantons are free to choose whether they want to implement birth allowances and how much they want to pay. During the last 50 years, 11 cantons have introduced a baby bonus, all increase the amount paid thereafter, and 2 cantons even abolish the baby bonus after all. This gives rise to a lot of cantonal variation. Thus, we use a difference-in-differences setting where we can analyze, due to several policy changes over time, both the introduction and the intensity of the treatment. Further, we employ a graphical event study analysis to study birth scheduling on the basis of daily birth counts. This allows us to understand whether parents are willing to shift births due to financial incentives.

We then combine these policy changes with administrative outcome data covering the universe of births, stillbirths and infant deaths in Switzerland from 1969 to 2017. On the one hand, we do not find evidence for birth scheduling. On the other hand, we find a

small and positive impact on fertility measures, a significant and sizable reduction of the stillbirth rate, and a significant but small increase in birth weight. While the latter effect is in absolute terms relatively small, the decline in the stillbirth rate is substantial. This robust reduction of the stillbirth rate, is especially strong for older or foreign mothers, and is almost exclusively driven by increases in the birth allowances in the early observation years (1969–1993).

All three chapters highlight the importance of studying alternative and indirect outcome measures in addition to the direct measures targeted by policy makers. In Chapter 1, for example, long-run maternal health effects of parental leave duration are clearly a side-effect as parental leave policies target mostly early child development and maternal labor market possibilities. As these policies are expensive to implement, the negative long-run health consequences of too long leave duration should be taken into account by policy makers. The same reasoning holds true for Chapter 2, where I argue that there is no additional benefit for the health of newborns if mothers can stay home and do not have to work during pregnancy due to longer parental leave for their first child. Finally, in Chapter 3 we only find little fertility effects, the directly targeted outcome measure, but a sizable and significant reduction in the stillbirth rate as well as a positive impact on birth weight, supporting the beneficial impact of birth allowances in Switzerland.



# Chapter 1

## Long-term Effects of Parental Leave

### Duration on Maternal Health:

## Evidence from Three Policy Changes in Austria

### 1.1 Introduction

There is substantial variation in parental leave policies across the globe. The difference across countries in leave length has even widened over the last decades. This is because most European countries tend to increase paid parental leave duration to levels of usually around a year. The United States, instead, offers only 12 weeks of unpaid leave. This is the ground for an ongoing debate on optimal leave policies. To feed into further debate, most research has focused on evaluating leave policies with respect to child development and maternal labor market outcomes. Maternal health, however, as an additional outcome, has mostly been neglected in the literature so far. This is surprising in light of the

worldwide increase of health costs.

This paper provides evidence on the effect of parental leave duration on maternal health in the short- and long-run. The few studies that have already focused on this topic hint toward diminishing returns to longer leave. A priori, it is not clear whether there might be a tipping point where longer leave duration could even become harmful for mothers. On top of that, mechanisms for improving or deteriorating maternal health might vary along the leave duration. While physical relaxation from giving birth and breastfeeding have been shown to be dominant drivers for improving health when leave is short, these forces are unlikely to continue to exist after a certain amount of months on leave (Chung et al., 2007; Dagher et al., 2014).

In this study, I investigate the effect of three parental leave reforms in Austria. Those reforms increased maternal leave from 1 year to 2 years in 1990; partially decreased maternal leave by six months to 1.5 years in 1996; and increased maternal leave again to 2.5 years in 2000. For the subset of mothers who live in Upper Austria, I can evaluate these policy changes with maternal health outcomes from 1998 to 2007. This allows me to observe mothers 1 year pre-birth up to 17 years post-birth when combining all three reforms. Health outcomes are observed in the Statutory Health Insurance Fund and provide information on all outpatient doctor visits including prescriptions and all inpatient hospitalizations.

I find a hump-shaped relationship between maternal health and leave length. The 1990 reform, which increased maternal leave by 1 year, substantially decreases medication costs. The 1996 reform, which decreased maternal leave by six months, does not show significant effects but the effect sign on costs is negative. This indicates that maternal health if anything improves. Finally, the 2000 reform, which increased maternal leave to 2.5 years, significantly increases the total outpatient costs. For all three reforms, there

seems to be a trade-off between inpatient and outpatient health outcomes. For both the 1990 and 2000 reform, inpatient health, measured in days of hospitalizations, significantly moves into the other direction than outpatient health.

An analysis of the type of medications prescribed and diagnoses when hospitalized reveals that especially medications prescribed for the nervous system and analgesics (painkillers) can explain this pattern. Diagnoses and medications related to cardiovascular health, though, are not significantly affected by any of the three reforms.

In a dynamic analysis, where I follow mothers over time, I can show that effects only start to become significant several years after birth. This may explain why studies that focused on outcomes measured relatively shortly after birth could not find any significant effects. Later on, effects continue to accumulate over time, which suggests that the full extent of the reforms may not even be completely evaluated at the end of my observation period.

A heterogeneity analysis reveals interesting additional patterns. Whether long or short leave is better for maternal health strongly depends on her own characteristics (both socioeconomic and health) and the health status of her child. Mothers with a high-socioeconomic status benefit less from an extension of parental leave to 2 years. The same holds true for married mothers. In the baseline analysis of the 1996 reform, where maternal leave declines, most of the effects are not significant. This is not the case for mothers with unhealthy babies (i.e. born preterm or with a low birth weight). These types of newborns may need extra time with their mothers and thus, a shorter leave may also be stressful for their mothers and deteriorate their health significantly along all dimensions. Finally, the 2000 reform, which increased leave substantially, is especially harmful for mothers who already suffered from mental disorders prior to giving birth. This indicates that a stable work environment could be crucial for their mental health.

The literature on parental leave and family health, especially maternal health, is relatively young. This literature emphasizes that the returns depend on several key features. First, the timing of measurement matters. Therefore, the effects on maternal health differ whether they are measured in the short- versus long-run. Second, the initial level of parental leave and the extent to which parental leave is increased are both key influencing factors. As such, an introduction is more beneficial than an increase at an already generous level of parental leave. Third, low-socioeconomic status (SES) mothers benefit more from generous parental leave settings than high-SES mothers.

More specifically, the three most related studies highlight the following facts: Baker and Milligan (2008b) evaluate an increase in parental leave in Canada from 6 to 12 months and find no significant effects on self-reported maternal health measured up to 2 years after giving birth. Beuchert et al. (2016), who study a reform in Denmark that increased maternity leave by on average 32 days, find small positive effects on the health of mothers who can be followed up to five years after giving birth. They show that benefits are higher for low-income families. Finally, the most recent study by Butikofer et al. (2018), which analyzes the introduction of paid parental leave in Norway and several extensions of it on maternal health at age 40, reports positive effects of the introduction and diminishing marginal returns on several health measures and health-promoting behavior.

Also the studies by Chatterji and Markowitz (2005), Chatterji and Markowitz (2012), and Guertzgen and Hank (2018) document the causal relationship of parental leave on maternal health. Furthermore, several public health studies document the correlation between maternity leave and mother's health as extensively summarized by Staehelin et al. (2007), Aitken et al. (2015), and Andres et al. (2016).

I contribute to this literature in various dimensions: I am the first to study increases and reductions in parental leave duration in one setting. This allows me to look at asym-

metries and to also explore the health gradient with respect to parental leave duration. Furthermore, I can combine all three reforms and, thus, study maternal health prior to birth and up to 17 years after birth. This allows me to understand the transition from an absence of significant effects in the short-run, to very large effects in the long-run. The very detailed data set that I am using also contains new outcome variables compared to the previous literature. I can analyze very precise outcome measures both in the inpatient as well as in the outpatient sector which are reported by the doctors themselves to the health insurance fund. This helps in understanding mechanisms that drive the self-reported health measures, which were mostly used in the previous literature.

Finally, my study is also closely related to the literature on the evaluation of parental leave policies and other outcomes, such as child development and maternal labor market outcomes as recently reviewed by Olivetti and Petrongolo (2017) and Rossin-Slater (2018).<sup>1</sup> All of these margins have to be taken into account by policy makers in order to find the optimal parental leave duration.

Understanding the relationship between maternal health and parental leave duration helps to guide policy makers. Showing that parental leave can actually be too long is of high policy relevance as parental leave is expensive for the society. Furthermore, the dependency of optimal parental leave length and characteristics of the newborn could be incorporated in future family policies, as well.

The rest of the paper is structured as follows. I first start by describing the institutional setting where I both explain the Austrian family policy system and the Austrian health care system. In Section 1.3, I describe the data and in Section 1.4 the empirical method.

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<sup>1</sup>The same Austrian reforms that I use in this study are also used in the following studies which evaluate other outcomes: Lalive and Zweimüller (2009) and Lalive et al. (2013), for example, study the effect on maternal labor market outcomes and fertility. Danzer and Lavy (2018) and Danzer et al. (2017) focus on cognitive outcomes of the affected children. More general, maternal labor market outcomes are also studied by Berger et al. (2005); Baker and Milligan (2008a); Schönberg and Ludsteck (2014) and child development is also studied by Ruhm (2000); Berger et al. (2005); Tanaka (2005); Baker and Milligan (2010); Liu and Skans (2010); Rasmussen (2010); Rossin (2011); Dustmann and Schönberg (2012); Carneiro et al. (2015); Dahl et al. (2016).

Section 1.5 presents the results. I start by showing the baseline results for all three reforms. I then look more deeply at the dynamics, analyze heterogeneous effects, conduct several sensitivity analyses, summarize the health gradient, and study subsequent reforms. A conclusion follows in Section 1.6.

## **1.2 Institutional Background**

In this section, I first describe the Austrian family policy system and especially the three reforms on parental leave duration that will be exploited in this paper. Then, I shortly discuss the Austrian health care system.

### **1.2.1 Austrian Family Policy System**

Family policies have a long history in Austria. Traditionally, Austria's child care is organized via a familialistic approach with relatively little professional child care institutions available (Dörfler and Blum, 2014). This led to giving a lot of value to the families themselves and to protecting pregnant women early on. Pregnant employed women were not allowed to work during the first four weeks after giving birth, while getting a financial compensation during this time period as early as 1888. The next 100 years were followed by many changes: introducing paid and mandatory pre-birth leave, extending paid and mandatory maternity leave, and introducing initially unpaid, ultimately paid compulsory leave up to the first birthday of the child.

Table 1.1 gives an overview of the most recent reforms in the Austrian parental leave system. In the following, I will describe in more detail the three reforms that occurred from 1st of July 1990 to 1st of July 2000, as they will be exploited in the empirical strategy.

Up to 30th of June 1990, mothers were entitled to eight weeks both pre- and post-

Table 1.1: Overview Austrian Parental Leave Reforms

Time period	duration (in months)	benefits (in €/ month)
until Jun 1990	12	340
Jul 1990–Jun 1996	24	340
Jul 1996–Jun 2000	18 (+6)	340
Jul 2000–Dec 2007	30 (+6)	340
Jan 2008–Dec 2009	30 (+6)	436
	20 (+4)	624
	15 (+3)	800
starting in Jan 2010	30 (+6)	436
	20 (+4)	624
	15 (+3)	800
	12 (+2)	1000
	12 (+2)	80% of last income

*Notes:* The information in this Table is based on Dörfler and Blum (2014). The months highlighted in parenthesis in Column (2) refer to those that are additionally and exclusively available to the second parent.

birth mandatory maternity leave, during which they got the full wage which they earned in the last quarter prior to giving birth. After mandatory leave, eligible mothers could choose to go on compulsory leave which lasted at maximum up to the first birthday of the child. Mothers were paid a flat benefit (roughly €340 per month) and were guaranteed job protection. First-time mothers over the age of 25 were eligible for compulsory leave if they had worked for at least 52 weeks in the 2 years preceding birth. The eligibility criteria is shorter for younger mothers and higher-order births.

As of 1st of July 1990, compulsory leave got extended up to the second birthday of the child and was defined as parental leave, so that fathers could take up to 6 months of the full parental leave duration. This policy reform was a result of parliamentary procedural requests which wanted to introduce paternal leave. Due to the flat benefit structure almost no fathers were taking up parental leave, which resulted in an increase of maternal leave from 1 to 2 years.

Out of budgetary constraints, six of the available 24 months of parental leave were

exclusively reserved for fathers for births that occurred after 1st of July 1996. This essentially reduced maternal leave from 2 to 1.5 years, leaving the flat benefit structure and job protection unchanged. As of 1st of July 2000, parental leave got extended to 36 months (reserving six months to fathers) in a broad reform, which changed the focus from employed mothers to all mothers. This reform was implemented in 2002, but was retroactively put in place for all employed mothers that gave birth after 1st of July 2000. Thus comparing employed mothers who gave birth before 30th of June 2000 versus after, leads to a difference in parental leave duration of 1 year, increasing it from 18 to 30 months available to mothers.

More recently, parents were given the opportunity to choose from a set of options, which combine different leave duration with flat benefit schemes. The last reform in 2010, also added a relatively short parental leave option with benefits that are depending on previous income. This option was specifically designed toward fathers to make leave more attractive in terms of monetary compensation.

### **1.2.2 Austrian Health Care System**

Austria's health insurance system is organized as a Bismarck model, characterized by universal health care coverage financed jointly by employers and employees. Depending on occupation and place of residence, individuals are insured with 1 out of 9 regional health insurance funds. This mandatory health insurance, based on employment and residency, leaves no choice for individuals with respect to the provider and the insurance package. The Upper Austrian Sickness Fund covers all inpatient and outpatient health services free of charge and the health care system is of high quality. Patients are free to consult a medical specialist, but an initial visit to a general practitioner for a referral to a specialist is recommended.



## 1.3 Data

My empirical analysis utilizes three separate administrative data sets: The Austrian Social Security Database (ASSD), the data from the Statutory Health Insurance Fund (SHIF) of Upper Austria, and the Austrian Birth Register (ABR). The social security database includes information on a daily basis on the full working history of the universe of private sector employees, birth events and parental leave spells.<sup>2</sup> The data also contain information on individual characteristics, such as the gender and age, and on job characteristics, such as general occupation classification and yearly earnings per employer. Based on an individual identifier the data can be linked to health outcomes for private sector employees residing in the state of Upper Austria from 1998 to 2007.<sup>3</sup> The data set includes information on all covered health services, such as health care utilization in the outpatient sector (i.e. doctor visits and prescriptions) and diagnoses in the inpatient sector. Finally, I complement these data with additional information on maternal characteristics at the time of birth (such as her education, marital status, and religion), the child's health (gestational length and birth weight), and additional birth characteristics (birth order and mode of delivery) from a matching procedure with the Austrian Birth Register.

In order to analyze the parental leave reforms and the effect on health, I consider the following outcome measures:

1. *Outpatient health-care expenditures.* Costs for all consultations with doctors and specifically general practitioners (GPs) and prescribed medical drugs from 1998 to 2007 are collected and measured in 2000 €.
2. *Prescriptions.* Information on prescription does not only include the financial costs but also the type of medication according to the Anatomical Therapeutic Chemical

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<sup>2</sup>The main collection purpose of this data is to document an employee's working history in order to calculate old age social security benefits.

<sup>3</sup>Upper Austria is one out of nine regions in Austria. It is with one sixth of Austria's total population the third most populous state.

(ATC) Classification System. With the help of this pharmaceutical coding system, I create dummy variables for ever having consumed analgesics, antidepressants, general nervous system medication, and cardiovascular system medication.

3. *Days of hospitalizations.* Each inpatient hospital visit is individually recorded in the data and the amount of overnight hospital stays is reported.
4. *Inpatient diagnoses.* The detailed reason for hospitalization is classified according to the ninth revision of the International Classification of Diseases and Related Health Problems (ICD-9). The causes are aggregated into 18 broad categories of which I will put a special emphasis on mental disorders, depression, and diseases of the circulatory system. These choices are guided by the previous literature on where to expect long-term effects of parental leave.
5. *Sick leave and Mortality.* Certain health outcomes, such as sick leave and mortality are recorded in the ASSD and, therefore, available for all mothers in my sample and not only those residing in Upper Austria. More specifically on the sick leave information; only sickness benefits above three days can be observed.<sup>4</sup>

I use accumulated annual measures at each point in time and at the end of the observed time window to have less noisy measures for health care expenditures, and to get information about health dynamics. In my baseline results, I only report estimates for the last point in time, i.e. accumulated outcomes over nine years.<sup>5</sup> Accumulated measures for diagnoses and prescriptions are coded such as *ever* diagnosed and, therefore, turn one in the first year of occurrence and stay one thereafter. All other measures (e.g. costs and

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<sup>4</sup>Absences below three days do not have to be reported and are, therefore, not reliably recorded. For more information on sickness benefits see Kuhn et al. (2009).

<sup>5</sup>As health outcomes are only available from 1998 to 2007 and as I estimate a difference-in-differences (DiD) regression discontinuity design (which will be explained in the next section), I lose 1 year of information for each individual. This is the result of the DiD because I want mothers to be comparable which means to have the same amount of years since giving birth. As an example, for the 1990 reform sample, I start to observe mothers eight years past giving birth, while I only start observing them nine years past giving birth for the 1989 sample. Therefore, I have to drop the first year for the 1990 sample and the last year for the 1989 sample.

days of hospitalizations) are accumulated and entail the full information on the amount. This means that in case of the 1990 (1996/2000) reform, I will show results for mothers who gave birth to their first child 16 (10/6) years ago. As health outcomes change over the life cycle, the results are not directly comparable across reform samples.

In the main analysis, I restrict my sample to first births and to those mothers that can be observed over the entire time window from 1998 to 2007. There are several reasons why mothers could leave the sample — mothers can become self-employed or mothers can move into one of the other regions in Austria.

*Descriptives.* Summary statistics for the described variables are presented in Table 1.2. The first column presents means and standard deviations for my main sample. The second to fourth columns restrict the attention to each sample used for one out of the three specific reforms. Column 2, for example, consists of all mothers giving birth in 1990 as well as the pre-reform year 1989 due to the RDD-DiD design which will be explained in more detail in the next section. Columns 3–4 are defined accordingly.

Mother's expenditures across the samples are not entirely comparable because the time since birth is shorter for more recent reform samples and, thus, mothers tend to be younger even with increasing age at time of birth. Additionally, in the third reform sample mothers are giving birth during the observation period and, therefore, tend to have more days of hospitalizations.

The average mother in the 1990/1996/2000 reform sample generated €2,423/2,371/2,354 of accumulated outpatient health expenditures at the end of the observation window and spent 8.23/7.59/15.13 days in hospital. 32.0/28.8/25.0 percent of mothers are getting prescriptions for diseases of the nervous system and sense organs, and 8.1/7.5/6.7 percent of mothers are hospitalized for mental health issues. General trends in motherhood can

Table 1.2: Descriptive Statistics

	Full sample	1989-1990 sample	1995-1996 sample	1999-2000 sample
Cumulated outpatient costs	2,383.1632 (3,146.199)	2,422.887 (3,612.154)	2,371.067 (3,252.893)	2,354.290 (2,391.621)
Cumulated GP costs	281.231 (458.955)	314.097 (606.927)	271.462 (372.244)	257.068 (350.763)
Cumulated medication costs	578.893 (2,497.664)	665.960 (2,753.755)	631.638 (2,750.641)	424.966 (1,806.091)
Cumulated days of hospitalizations	10.144 (5.017)	8.227 (5.575)	7.589 (4.948)	15.126 (4.425)
Mental disorders	0.075	0.081	0.075	0.067
Depression	0.065	0.072	0.065	0.059
Antidepressants	0.150	0.172	0.150	0.126
Nervous system medication	0.287	0.320	0.288	0.250
Analgesics	0.130	0.142	0.138	0.108
Circulatory system diseases	0.013	0.014	0.013	0.012
Cardiovasc. system medication	0.074	0.098	0.072	0.050
Maternal age at birth	27.333 (4.854)	26.180 (4.663)	27.518 (4.688)	28.362 (4.971)
High-wage at birth	0.551	0.549	0.544	0.562
Married at birth	0.702	0.742	0.705	0.655
Low birth weight baby	0.040	0.040	0.037	0.042
Preterm birth	0.037	0.033	0.038	0.041
Caesarean section	0.121		0.107	0.137
Mental disorder pre-birth	0.014			0.014
Observations	39,858	13,313	14,162	12,383

*Notes:* The full sample covers all births that occur in the reform (1990, 1996, 2000) and the preceding non-reform (1989, 1995, 1999) years in the months from April to September (a bandwidth of three months chosen in the main analysis). Columns 2–4 restrict the sample to each one of the reform and the preceding non-reform year. Standard errors are shown in parentheses if necessary. Due to data availability Caesarean sections are only available starting from 1995 and Mental disorders pre-birth starting from 1999.

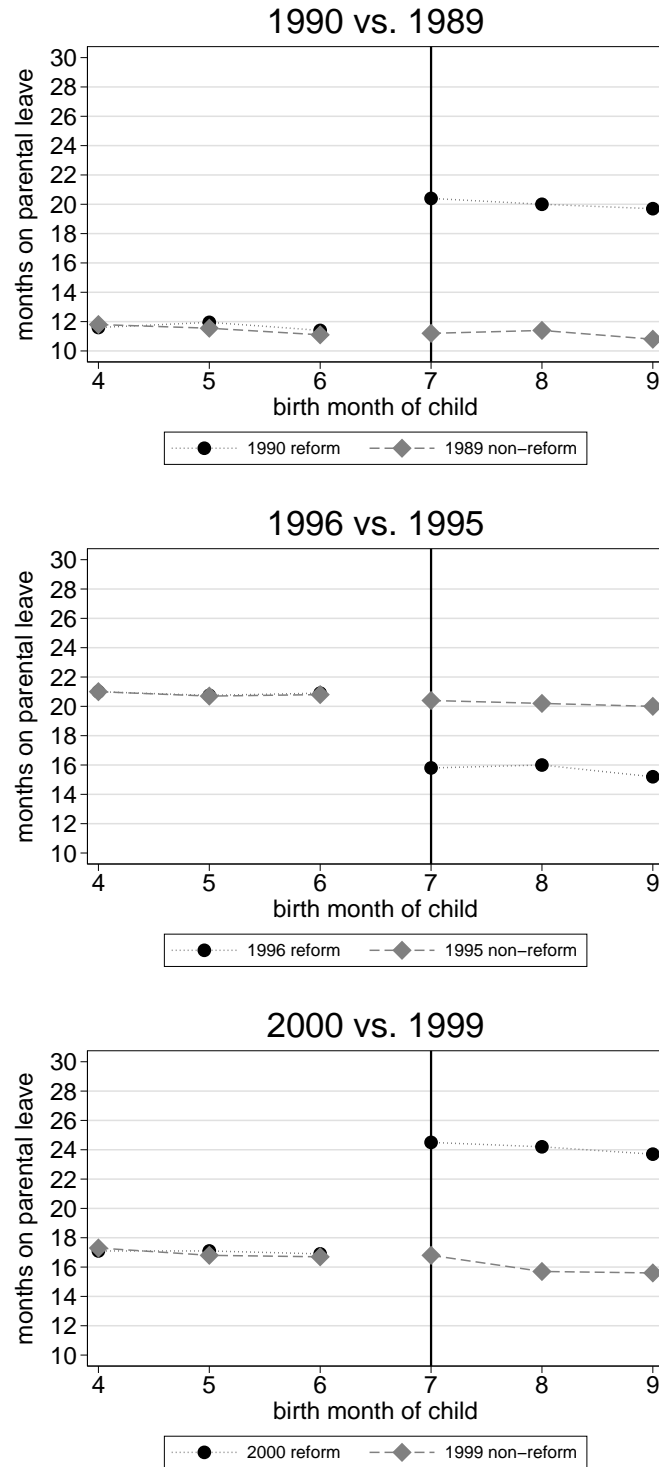
also be observed: Maternal age at first birth increases from 26.2 years in the 1990 reform sample to 28.4 years in the 2000 reform sample; and mothers are less likely to be married (65.5 percent) in the last sample. The health of the newborn is relatively constant over time (roughly 3–4 percent of babies are born preterm or with low birth weight).

## 1.4 Empirical Method

In this paper, I assess the average causal effect of duration of parental leave on maternal health in the short- to long-run. To do this, the estimation strategy relies on exogenous variation in leave duration based on the three described reforms. Namely, parental leave increased by 1 year to 2 years for mothers giving birth after 1st of July 1990. This was partially reversed to 1.5 years in July 1996 and got extended again to 2.5 years in July 2000. Figure 1.1 depicts how these changes in leave duration affected actual behavior, i.e. the months on parental leave. Black dotted lines describe reform years and gray dashed lines depict non-reform years preceding a reform. In all three reform years, mothers who give birth in July show a significant jump in months on parental leave. For example, mothers who gave birth in July 1990 versus June 1990 stay on leave for an average of 21 versus 11 months. July 1996 mothers are five months less on leave than June 1996 mothers, and July 2000 mothers stay on leave for more than 27 months versus 18 months as the June 2000 mothers. In non-reform years, there is no discontinuity.

This sharp discontinuous jump in leave duration at the policy reform thresholds allows me to employ a regression discontinuity design. However, as systematic differences in maternal characteristics by season of birth have been shown to exist (Currie and Schwandt, 2013), I will estimate a difference-in-differences regression discontinuity design (DiD-RDD). This approach allows to control for differences in outcomes between mothers who gave birth after versus before the reforms in July that are unrelated to

Figure 1.1: Discontinuity in Actual Parental Leave Duration



*Notes:* This Figure shows the average amount of months mothers spend on leave by birth month of the child. Black dotted lines refer to the reform years and, therefore, depict a discontinuity in July when the policy reforms were implemented. Instead, grey dashed lines refer to the preceding non-reform year and are, therefore, stable across the year. Vertical black lines inform about the month in which the policy changes were implemented.

the reform itself. I estimate the following equation separately for each reform sample on eligible women using a bandwidth of three months:<sup>6</sup>

$$\begin{aligned}
Y_{it} = & \beta_0 + \beta_1 T_i * reform_i + \beta_2 T_i + \beta_3 reform_i + \beta_4 R_i * reform_i + \beta_5 R_i \\
& + \beta_6 T_i * R_i * reform_i + \beta_7 T_i * R_i + \epsilon_i,
\end{aligned}
\tag{1.1}$$

with  $Y_{it}$  the health outcomes of mother  $i$ ,  $t$  years after giving birth to her child;  $T_i$  an indicator for giving birth after the policy change (such that  $T=1$  for July-September mothers and  $T=0$  for April-June mothers);  $R_i$  the number of months from the policy reform (the so called rating variable, in order to allow for separate trends on each side of the cutoff); and  $reform_i$  a dummy for a reform year (such that  $reform_i=0$  in a pre-reform year and  $reform_i=1$  in a reform year). Here,  $\beta_1$  identifies the net effect of changing parental leave length controlling for the difference between the outcomes of mothers who give birth in the second half versus the first half of a reform year relative to the difference of these mothers in a non-reform year preceding the policy change.

*Identification.* There are three identifying assumptions in this setting: 1) There has to be a sharp treatment discontinuity at the cutoff (Lee and Lemieux, 2010a); 2) Instead, there should be no discontinuity in potential outcomes at the cutoff (Lee and Lemieux, 2010a); 3) Due to adding the differences-in-differences structure, I have to assume that month-of-birth effects do not vary across years. Assumption (1) is fulfilled as shown in Figure 1.1. There are standard procedures to validate the intestable assumption (2). I test for discontinuities in non-outcome variables, which I report in Table A.1 in the Appendix<sup>7</sup> and conclude that as these variables evolve relatively smoothly across the

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<sup>6</sup>I estimate local linear regressions with a rectangular kernel (as suggested by Imbens and Lemieux (2008)), cluster standard errors by birth month (as suggested by Lee and Card (2008)), and choose the bandwidth implementing a cross-validation method (as recommended by Imbens and Lemieux (2008)).

<sup>7</sup>All following Figures and Tables denoted by A are reported in the *Appendix: Chapter 1*.

cutoff, RD estimates are likely to generate consistent estimates.<sup>8</sup> Assumption (3) seems relatively mild and reasonable, as I am only using the pre-reform year. It is very unlikely that month-of-birth effects change over such a short time horizon.

## 1.5 Results

In this section, I start by reporting the baseline results. Based on these estimates, I focus on more specific causes and prescriptions. As I can exploit the panel structure of the data and follow mothers over time, I report health dynamics to understand how the baseline results evolve over time and at what time after birth significant effects emerge. These results will be completed by a heterogeneity analysis and several robustness checks including placebo estimations, playing with different bandwidth choices, and adding some health measures from the ASSD sample which are available for all Austrian mothers.

### 1.5.1 Baseline Results

I present estimates of each reform on different general maternal health outcomes in Table 1.3. The general health outcomes that I consider are: The 9-years accumulated health costs in the total outpatient sector, the costs when visiting a general practitioner, the costs concerning prescribed medications, and the accumulated number of days of hospitalizations at the end of the observation period. The RDD-DiD estimates, based on Equation (1.1), report the difference in health outcomes for mothers giving birth to their first child after the policy change. These estimates are unrelated to seasonality, as I difference out seasonality effects by including the pre-reform year. Standard errors are clustered at the level of the running variable as suggested by Lee and Card (2008).

I start by discussing the results for the reform in 1990, the year where parental leave

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<sup>8</sup>Some of the estimates show a significant jump around the cutoff. This jump is usually very small in absolute size and thus unlikely to bias the results.



Table 1.3: Estimates on General Health Outcomes

Dependent variable	Outpatient costs	GP costs	Medication costs	Days of hospitalizations
<b>1990 Reform</b>				
RDD-DiD	−290.6* (140.2)	41.44 (36.45)	−337.4*** (70.27)	2.764** (0.817)
Mean of Dep. Var.	2422.887	314.097	665.960	8.227
Observations	13,313	13,313	13,313	13,313
<b>1996 Reform</b>				
RDD-DiD	−275.3 (170.2)	−11.02 (21.89)	−350.8* (163.0)	0.520 (0.765)
Mean of Dep. Var.	2371.067	271.462	631.638	7.589
Observations	14,162	14,162	14,162	14,162
<b>2000 Reform</b>				
RDD-DiD	255.1** (96.15)	−36.31*** (5.540)	171.6 (91.91)	−3.718** (1.260)
Mean of Dep. Var.	2354.290	257.068	424.966	15.126
Observations	12,383	12,383	12,383	12,383

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1). Each reform is individually estimated. The sample includes all mothers within a bandwidth of three months in a reform year and the preceding non-reform year, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 17/11/7 years ago for the reform in 1990/1996/2000.

got extended by 1 year to 2 years. Outcomes are accumulated over the 9-year window and health outcomes of mothers are evaluated 17 years after giving birth. GP costs are not significantly affected by the reform. Generally, the two strongest cost drivers of significantly reduced outpatient costs are the GP and the medication costs. For the 1990 reform, medication costs are significantly lower for post-reform mothers. The point estimate of €-337.4 is very large in absolute values. At the mean, this corresponds to reducing costs related to prescribed medication by half. This positive health impact by reducing medication costs is partly offset by the significant increase in days of hospitalizations. Post-reform mothers spend 2.8 days more in a hospital, which corresponds to an increase of 34 percent, at the mean. As in the case of hospitalizations, costs are not

reported in the data, these two effects cannot directly be compared. Thus, there seems to be a trade-off between better outpatient and worse inpatient health outcomes.

The 1996 reform, which decreased parental leave from 2 years to 1.5 years for mothers, leads to a marginally significant reduction in the outpatient health outcomes. The weakly significant effect of reducing medication costs by €350.8, is again non-negligible. The effect on days of hospitalizations is both very small and not significant. These outcomes are measured 12 years after giving birth. Therefore, estimates cannot be directly compared to the 1990 reform, where outcomes are measured 16 years post-birth. Later, when I directly compare the reforms with each other, I adjust sample windows to overlapping event windows.

Lastly, in the lower part of Table 1.3, I report RDD-DID estimates for the 2000 reform. This reform increased parental leave again by 1 year to a total duration of 2.5 years. This reform significantly increases overall outpatient costs by €255.1 — an increase of around 11 percent, at the mean. This overall increase of total outpatient costs is a combination of significantly decreased GP costs by €-36.31 and a non-significant increase of medication costs by €171.6. In line with the 1990 reform, also for the 2000 reform outpatient health outcomes move in the opposite direction than inpatient health outcomes. Specifically, the estimate on days of hospitalizations opposes the increase in health costs in the outpatient sector with a decline of 3.7 days, a reduction of 25 percent, at the mean.

Tables 1.4–1.5 report more specific health outcomes. Table 1.4 shows estimates on mental health outcomes both in the inpatient (mental disorders and depression) as well as in the outpatient (antidepressants, nervous system medication, and analgesics) sector and Table 1.5 looks more specifically at cardiovascular health outcomes, such as general hospital diagnoses categorized as circulatory system diseases and prescriptions related to the cardiovascular system. Both Tables are structured as Table 1.3.

Table 1.4: Estimates on Mental Health Outcomes

Dependent variable	Mental disorders	Depression	Anti-depressants	Nervous system med.	Analgesics
<b>1990 Reform</b>					
RDD-DiD	0.0283** (0.0105)	0.0187 (0.0121)	-0.00374 (0.0201)	-0.00855 (0.00925)	0.0292*** (0.00555)
Mean of Dep. Var.	0.081	0.072	0.172	0.320	0.142
Observations	13,313	13,313	13,313	13,313	13,313
<b>1996 Reform</b>					
RDD-DiD	-0.0250* (0.0123)	-0.0174 (0.0112)	-0.0127 (0.0164)	0.00631 (0.0211)	-0.0276** (0.00994)
Mean of Dep. Var.	0.075	0.065	0.150	0.288	0.138
Observations	14,162	14,162	14,162	14,162	14,162
<b>2000 Reform</b>					
RDD-DiD	0.0104 (0.00834)	0.00418 (0.00804)	0.0260* (0.0105)	0.0702*** (0.0103)	0.0504*** (0.00918)
Mean of Dep. Var.	0.067	0.059	0.126	0.250	0.108
Observations	12,383	12,383	12,383	12,383	12,383

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1). Each reform is individually estimated. The sample includes all mothers within a bandwidth of three months in a reform year and the preceding non-reform year, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 17/11/7 years ago for the reform in 1990/1996/2000.

For estimates on mental health outcomes, the pattern seems to direct toward longer leave being harmful for maternal mental health. Namely, for the 1990 reform, there are two significant estimates: One on the probability of ever being diagnosed with a mental disorder and the other one on the likelihood of being prescribed an analgesics. The estimate on mental disorders is 0.028 and the one on analgesics is 0.030. As such, they both are positive and therefore tend toward a negative health impact of the 1990 reform, which increased parental leave by 1 year. Contrary, the 1996 reform which decreased leave by six months decreases the probability of analgesics prescriptions by 0.028 and the one on being diagnosed with a mental disorder by 0.025. The 2000 reform, which increases parental leave by 1 year again to 2.5 years, increases antidepressants by 0.026,

Table 1.5: Estimates on Cardiovascular Health Outcomes

Dependent variable	Circulatory system diseases	Cardiovascular system medication
<b>1990 Reform</b>		
RDD-DiD	−0.00404 (0.00303)	0.000722 (0.0160)
Mean of Dep. Var.	0.014	0.098
Observations	13,313	13,313
<b>1996 Reform</b>		
RDD-DiD	0.0128*** (0.00316)	−0.0122 (0.00868)
Mean of Dep. Var.	0.013	0.072
Observations	14,162	14,162
<b>2000 Reform</b>		
RDD-DiD	0.00298 (0.00484)	0.00521 (0.00624)
Mean of Dep. Var.	0.012	0.050
Observations	12,383	12,383

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1). Each reform is individually estimated. The sample includes all mothers within a bandwidth of three months in a reform year and the preceding non-reform year, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 17/11/7 years ago for the reform in 1990/1996/2000.

nervous system medications by 0.070, and analgesics by 0.050. These effects are large in magnitude, as they can be interpreted as an increase of 21 percent, 28 percent, and 47 percent, at the mean, respectively. Summarizing the mental health outcomes, one can clearly say that it is the outpatient sector for mental health outcomes that reacts to a change in parental leave duration. Longer leave deteriorates maternal mental health with more prescriptions for the nervous system and analgesics.

For cardiovascular health outcomes, which are reported in Table 1.5, only one point estimate is significant. For the 1996 reform, which decreased parental leave by six months to 1.5 years, there is a positive estimate on the probability of ever being hospitalized with

a disease of the circulatory system of 0.013. This effect is contrary to what is observed with mental health outcomes, as in this case the shorter leave leads to worse inpatient health.

Overall, also having in light the main results reported in Table 1.3, there seems to be a general trade-off between inpatient and outpatient health, which also gets confirmed when looking closer at the mental health and cardiovascular health outcomes.

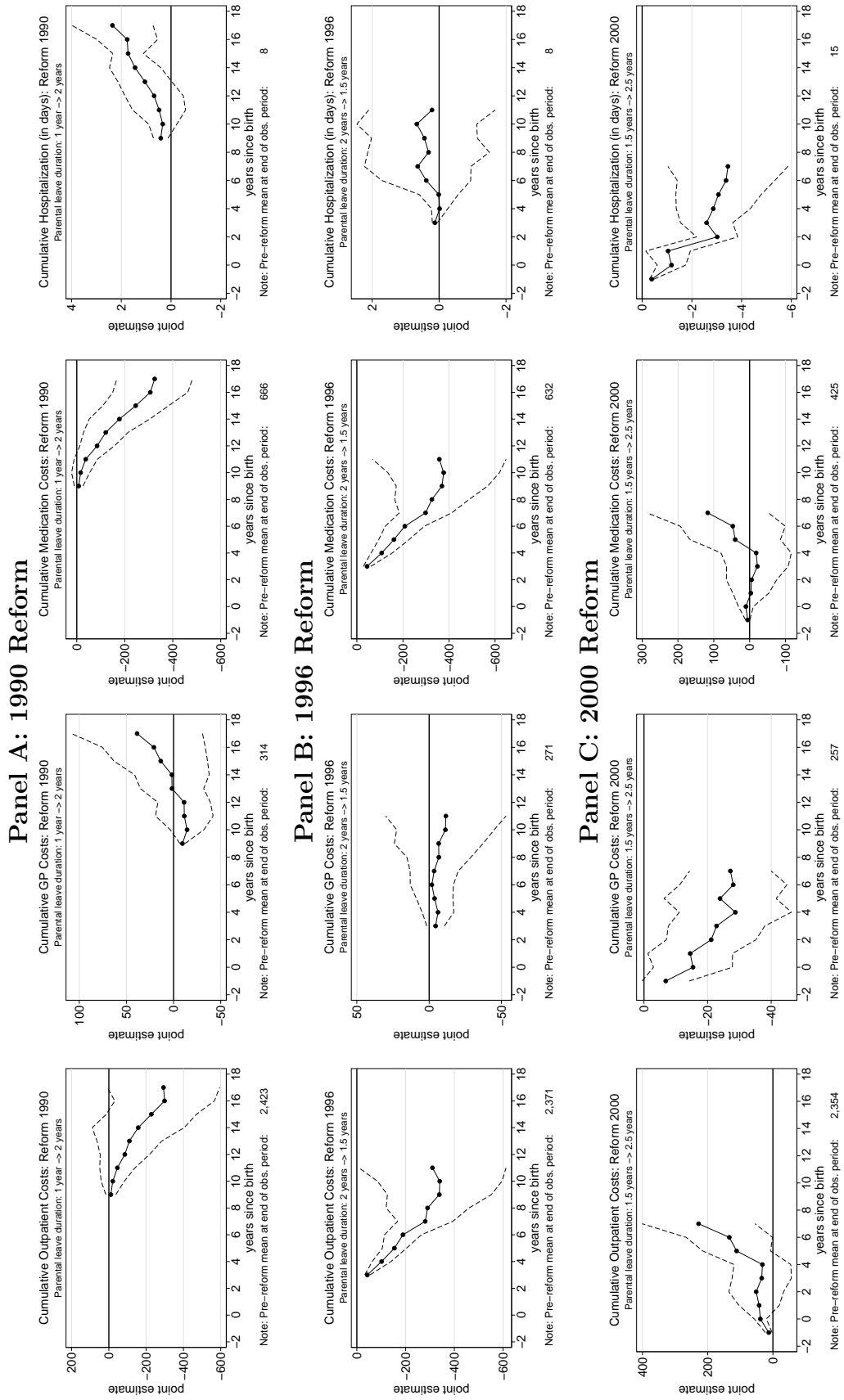
As a next step, it is interesting to see whether the inpatient and outpatient health outcomes diverge from the early beginning on or whether there are other observable dynamics. In the next section, I look more specifically at how these outcomes evolve over time.

### 1.5.2 Health Dynamics

Figure 1.2 reports the dynamics of the general health outcomes over time and shows an estimate for each observed year. Results are shown by each point in time accumulated up to this moment to see whether effects happen quickly after giving birth or if they increase over time. Thus, the last dot in each subfigure corresponds to the estimate reported in the Tables before. Additionally, confidence intervals are shown with dashed lines.

Panel A reports the results for the 1990 reform. Due to the availability of the data, one can observe accumulated health outcomes from 9 to 17 years after giving birth. It can be seen that the negative effect on outpatient costs mostly driven by the medication costs starts right at the beginning of the observation period and increases until 17 years after giving birth. The effect on the inpatient health outcome, measured with days hospitalized, follows the same logic but in the other direction. Already 9 years after giving birth, mothers who were on leave for 1 more year, are more likely to be hospitalized and this estimate increases over time and starts to become significant 13 years after giving birth.

Figure 1.2: Health Dynamics



*Notes:* This Figure shows point estimates from estimating Equation (1.1) on different accumulated health outcomes. Each dot represents a separate estimation on accumulating the health outcome up to this point in time. Dashed lines report 95 percent confidence intervals.

The dynamics for the 1996 reform are reported in Panel B. Mothers both pre- and post-reform do not differ in their likelihood of being hospitalized, but health costs in the outpatient sector show different patterns. Mothers in the post-reform sample, who spent on average six months less on parental leave than pre-reform mothers, have less expenditures on general outpatient costs and especially medication costs. This effect is significant already three years after giving birth, which is the first year where mothers can be observed in the panel. The effect is especially precisely estimated from three to six years after giving birth, but continues to increase in absolute size in the years thereafter.

Finally, for the 2000 reform as reported in Panel C, one can observe mothers already 1 year *before* giving birth. Importantly, one has to keep in mind that pre-reform versus post-reform mothers are generally on leave for 1.5 versus 2.5 years, respectively. As such, it is interesting to see, whether the health outcomes diverge while post-reform mothers are still on leave and pre-reform mothers started working already, i.e. in the years 1.5 to 2.5 after giving birth. This does not seem to be the case. Focusing on the outpatient and medication costs, which have the strongest effects in absolute terms, one can clearly see, that health outcomes only start to diverge five years after giving birth and then constantly increase up to 7 years after giving birth. For these outcomes, in line with the RDD argument, these mothers also do not differ pre-birth. For the days hospitalized, instead, mothers differ already pre-birth which makes the interpretation of the estimate on days hospitalized more difficult. The same is true for GP costs, but these are very small in absolute size.

The general take-away from looking at the dynamics is that health effects accumulate over time. Only looking at the time period in which some mothers are still on leave and others started working does not fully capture all the health effects of parental leave reforms. This can also explain the effects that have already been found in the literature.

E.g. Baker and Milligan (2008b) focused on health outcomes 1 year after birth and could not detect any significant effects. However, this may just have been a too short time window. Instead, the strong and positive effects of longer leave reported by Butikofer et al. (2018) at the age of 40 may capture the fact, that the effects on maternal health increase over time and may even be higher at older ages. Mothers analyzed with the 1990 reform, for example, are on average 43 years old in my analysis. Even these mothers still show a positive slope of the health effects, suggesting that health effects are still increasing at that age.

### 1.5.3 Heterogeneity

Next, I examine whether the effects of the reform varied with characteristics of the mothers (wage or marital status), the health of the newborn (born with low birth weight or preterm), the delivery method (Caesarean section) for the two latter reforms, and pre-birth mental health for the 2000 reform. Results are reported in Tables 1.6–1.8.

Specifically, I augment Equation (1.1) by interacting all parameters with one of the heterogeneous subgroups and report only the parameters of interest, i.e. the effect of the policy change in a reform year and the interaction term. The interaction term reports the additional effect (added to the baseline RDD-DiD estimate) for the stated heterogeneous group, i.e. for high-wage mothers in the first panel.

Table 1.6 reports the effects for the 1990 reform. The results by wage show a clear pattern. While all significant effects show better health outcomes (negative point estimates) for low-wage mothers, the interaction term for these outcomes is positive and significant. Additionally, in absolute size, the interaction term is larger than the effect for low-wage mothers, so that the overall effect on maternal health for high-wage mothers is positive but insignificant. Thus, the additional year of parental leave is beneficial for low-wage



mothers only. This is not surprising, as high-wage mothers might find it easier to arrange child care and probably also benefit more from working due to higher wages. The same argumentation carries on when looking at the sign of the effect for married versus unmarried mothers. Unmarried mothers benefit substantially more from longer parental leave than married mothers.

Table 1.6: Heterogeneous Effects: 1990 Reform

Dependent variable	Outpatient costs	GP costs	Medication costs	Days of hospitalizations
<b>High-wage vs low-wage</b>				
RDD-DiD	−984.4** (318.6)	0.948 (64.01)	−887.1*** (170.4)	4.064* (1.834)
Interaction high-wage	1175.3** (306.1)	74.79 (47.46)	923.3*** (188.1)	−1.853 (1.678)
<b>Married vs unmarried</b>				
RDD-DiD	−872.7*** (189.6)	−25.32*** (6.225)	−594.6** (169.4)	1.910*** (0.385)
Interaction married	787.1*** (67.36)	91.14 (50.71)	349.7* (155.2)	1.139 (0.608)
<b>Low birth weight baby vs normal birth weight baby</b>				
RDD-DiD	−294.8 (155.6)	42.38 (42.31)	−334.8*** (66.82)	2.197* (0.945)
Interaction LBWB	−160.7 (417.5)	−24.78 (214.1)	−290.2 (176.9)	13.42** (4.819)
<b>Preterm birth vs normal birth</b>				
RDD-DiD	−259.1 (187.7)	39.71 (43.45)	−319.8** (99.26)	3.074* (1.265)
Interaction preterm	−1092.3 (1595.7)	61.68 (250.7)	−626.0 (882.6)	−9.230 (16.61)
Mean of Dep. Var.	2422.887	314.097	665.960	8.227
Observations	13,313	13,313	13,313	13,313

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1) and an interaction with the stated heterogeneous group. The sample includes all mothers within a bandwidth of three months in the reform year 1990 and the preceding non-reform year 1989, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 17 years ago.

Table 1.7: Heterogeneous Effects: 1996 Reform

Dependent variable	Outpatient costs	GP costs	Medication costs	Days of hospitalizations
<b>High-wage vs low-wage</b>				
RDD-DiD	−27.79 (340.1)	25.76 (38.53)	−272.3 (181.4)	5.028* (2.368)
Interaction high-wage	−480.4 (584.1)	−66.61* (30.20)	−159.8 (385.5)	−7.914** (2.920)
<b>Married vs unmarried</b>				
RDD-DiD	−75.49 (401.5)	−14.48 (37.87)	−237.8 (325.2)	−0.942 (2.125)
Interaction married	−250.8 (341.1)	5.767 (30.84)	−140.1 (238.5)	2.298 (2.649)
<b>Low birth weight baby vs normal birth weight baby</b>				
RDD-DiD	−445.4** (158.2)	−16.17 (23.18)	−488.2** (136.9)	0.884 (0.968)
Interaction LBWB	4502.1*** (319.3)	154.5 (88.92)	3596.3*** (668.9)	−6.138* (2.798)
<b>Preterm birth vs normal birth</b>				
RDD-DiD	−286.7 (148.1)	−16.44 (22.81)	−344.3* (147.7)	1.803 (1.113)
Interaction preterm	304.8 (586.1)	136.4*** (14.57)	−173.4 (470.1)	−32.68*** (7.756)
<b>Caesarean section vs vaginal delivery</b>				
RDD-DiD	−168.8 (126.9)	1.616 (16.83)	−298.6* (118.9)	1.478* (0.660)
Interaction CS	−1099.1* (446.1)	−127.9* (50.44)	−565.9 (526.1)	−9.916** (2.548)
Mean of Dep. Var.	2371.067	271.462	631.638	7.589
Observations	14,162	14,162	14,162	14,162

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1) and an interaction with the stated heterogeneous group. The sample includes all mothers within a bandwidth of three months in the reform year 1996 and the preceding non-reform year 1995, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 11 years ago.

Table 1.8: Heterogeneous Effects: 2000 Reform

Dependent variable	Outpatient costs	GP costs	Medication costs	Days of hospitalizations
<b>High-wage vs Low-wage</b>				
RDD-DiD	604.5*** (119.1)	7.406 (9.847)	453.6*** (76.14)	−3.073 (1.747)
Interaction high-wage	−612.6*** (101.2)	−76.06*** (11.87)	−494.3*** (53.68)	−1.157 (1.057)
<b>Married vs unmarried</b>				
RDD-DiD	729.3** (214.7)	−50.47** (19.53)	545.1* (236.1)	−4.367* (2.043)
Interaction married	−724.6** (185.5)	22.35 (29.93)	−573.5* (223.2)	1.049 (1.222)
<b>Low birth weight baby vs normal birth weight baby</b>				
RDD-DiD	332.8** (103.6)	−21.96** (7.017)	199.3* (97.60)	−2.752* (1.078)
Interaction LBWB	−1693.7*** (130.8)	−314.6*** (28.86)	−610.9*** (127.8)	−21.03*** (2.832)
<b>Preterm birth vs normal birth</b>				
RDD-DiD	245.8* (98.63)	−30.06*** (4.181)	152.6 (102.3)	−3.200** (0.982)
Interaction preterm	283.3** (74.28)	−143.7** (51.27)	474.0 (292.5)	−9.274 (4.953)
<b>Caesarean section vs vaginal delivery</b>				
RDD-DiD	191.0** (64.34)	−25.46*** (4.908)	100.4 (63.63)	−1.445 (1.131)
Interaction CS	489.7 (389.3)	−82.64* (37.06)	552.8* (236.9)	−14.74*** (1.498)

*Notes:* This Table continues on the next page.

Table 1.8 continued: Heterogeneous Effects: 2000 Reform

Dependent variable	Outpatient costs	GP costs	Medication costs	Days of hospitalizations
<b>Mental disorder pre-birth vs healthy pre-birth</b>				
RDD-DiD	198.3* (88.58)	-40.94*** (4.999)	161.7 (84.99)	-3.847** (1.118)
Interaction MDPB	3167.0*** (361.7)	282.2*** (35.55)	436.5 (541.8)	4.366 (13.58)
Mean of Dep. Var.	2354.290	257.068	424.966	15.126
Observations	12,383	12,383	12,383	12,383

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1) and an interaction with the stated heterogeneous group. The sample includes all mothers within a bandwidth of three months in the reform year 2000 and the preceding non-reform year 1999, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 7 years ago.

Next, I examine the effect of the 1990 reform by the health of the newborn. I create two indicators; (1) if the child is born with less than 2,500 grams — which is defined as a low birth weight baby and (2) if the child is born before the 37th week of gestation — which determines a preterm birth.<sup>9</sup> These two dummy variables potentially capture different things. A preterm baby may well be mature enough to survive outside the mother's body, while a low birth weight baby may have spent up to 40 weeks in a mother's womb but did not grow enough. Thus, a low birth weight baby will generally be put in neonatal care to gain weight. Unhealthy babies may demand an extra amount of maternal care which may last into later childhood. As such, one would expect mothers from unhealthy babies to benefit more from longer leave, which is partly what the point estimates confirm. Mothers with unhealthy babies show overall a stronger decline in the cost health measures than mothers with healthy babies. The interaction term is, though, never significant, which is most likely the result of the small sample size for these subgroups: Roughly 3-4 percent of all births are either low birth weight or preterm.

<sup>9</sup>Both thresholds of a low birth weight baby and a preterm birth are defined by the World Health Organization.

For the 1996 reform, results are reported in Table 1.7. Most of the point estimates are generally not significant, but overall support the previous interpretations. While estimates generally point toward a non-significant reduction of health costs, this effect is stronger for high-wage and married mothers. The result for mothers with low birth weight babies is very interesting. Mothers with healthy babies are facing lower costs (i.e. €-445.4 on outpatient costs and €-488.2 on medication costs), but for mothers with unhealthy babies the reduction in parental leave to 1.5 years is very harmful (i.e. +€4502.1 on outpatient costs and +€3596.3 on medication costs). Thus, for mothers with babies who need some extra care the reduction to shorter leave is also very stressful for mothers and, therefore, decreases maternal health. Finally, as birth delivery methods are recorded from 1995 onward, I can also distinguish by Caesarean section. Generally, mothers who gave birth via Caesarean section benefit more from shorter leave. This is at first surprising, as in general mothers with a Caesarean section need more time to recover from giving birth. However, in the Austrian case parental leave is initially already long enough so that this usual mechanism is not playing a key role here.

In Table 1.8, I show heterogeneous effects for the 2000 reform. Long parental leave almost exclusively shows to be beneficial for mothers with low birth weight babies. All health measures become significantly reduced by a large amount. All other types of mothers show deteriorated or unchanged health outcomes. While I can observe health measures pre-birth for the reform in 2000, I also distinguish by mothers who already showed mental health problems before giving birth. Mothers with mental health difficulties pre-birth are especially prone to worse health in the medium-run due to longer leave. The point estimate on the interaction term on outpatient health of €3167.0 is very large and significant. At the mean, this estimate corresponds to more than a doubling. The same holds true for GP costs and medication costs, while the latter effect is not significant.

Furthermore, I also conduct subgroup analyses by tenure and work experience. For brevity they are not reported here. I study different thresholds of tenure (3/5/10 years) and experience (5/10/15 years). In general, mothers with shorter tenure benefit from longer leave. This confirms the previous results as short tenure might again proxy for an unstable environment. Additionally, mothers with long work experience tend to be unaffected by any of the policy changes.

Altogether, the heterogeneity analysis reveals insights which are of great importance for policy makers. The results suggest that parental leave can be too long. This is especially the case for high-wage and married mothers who seem to be in a stable environment. Additionally, mothers with a bad mental health before birth, are especially prone to be harmed by long leave. This type of mothers might need a stable work environment and a long absence from work may deteriorate their health tremendously. Contrary, mothers with unhealthy babies and to some extent also mothers with low-SES benefit from longer leave which should be taken into account when entitling women to parental leave.

#### **1.5.4 Robustness**

I conduct several robustness analysis. I start by reporting the results of a placebo analysis where I assume a reform in July 1992 or another one in July 1994. This sensitivity check is reported in Table A.2 and shows no significant effect on any of the health outcome measures for the 1994 reform and significant but very small effects for the 1992 reform. Most of these significant estimates are economically not meaningful. Overall, this supports the evidence that the previously reported estimates, are the result of the reform changes in 1990, 1996, and 2000.

Table A.3 shows estimates on general health outcomes, where I additionally control for maternal characteristics, such as her age, marital status, origin, and wage. In a standard

RDD setting, including these controls should not matter, however, as not all of these controls varied smoothly around the cutoff, I report this additional specification. The results are very similar to the previous stated ones and thus confirm the validity of the RDD in this context.

In Tables A.4–A.6, I report the robustness to different choices of bandwidths. The reported results show how estimates on the general health outcome measures vary from choosing a bandwidth of 3 months up to 6 months. This exercise is each done individually by reform.<sup>10</sup> In general, the sign of the effects, and thus the interpretation of each policy change, stays relatively constant over all choices of bandwidths.

Finally, I can also show the long-term effect of all three reforms on health outcomes that are observable for all Austrian mothers who are working. These results are reported graphically in Figure A.1, where I leave the empirical method unchanged as in Equation (1.1) but include the full sample and not only those mothers who reside in Upper Austria. Due to the continuous availability of the data over the entire reform period, I can now compare all three reforms over the time period from 2 years pre-birth up to 14 years post-birth. I show estimates for sickness days and mortality. The latter is a very severe health measure and occurs seldomly and is, thus, a noisy measure.

The estimates on sickness days are most of the time insignificant. If anything, they support the previous results for the 2000 reform which significantly deteriorated outpatient maternal health. Also for the full Austrian sample, there seems to be a tendency for more sickness days if facing longer parental leave. The same holds true for mortality. Both the 1990 and 2000 reforms seem to increase mortality. Especially for the 2000 reform, this effect is sizable as it more than doubles mortality if evaluated at the pre-reform mean. Overall, the harmful effect of the 2000 reform is also confirmed by this additional analysis.

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<sup>10</sup>For all estimations I leave the functional form unchanged and estimate a so called linear interaction model, where I allow for linear trends to be different before and after the reform.

### 1.5.5 Health Gradient

This section discusses the general take-away from studying the three reforms. Specifically, I adjust the previous analysis in order to be able to compare the 1990 and 1996 reforms, and the 1996 and 2000 ones, directly. To do so, I focus on the years 9 to 11 after giving birth for the first two reforms and the years 3 to 7 for the two latter reforms. This adjustment enables having two overlapping time windows so that one can also say something about the health gradient. These results are reported in Figure 1.3.

In Panel A, where I directly compare the 1990 and 1996 reforms, results are now more noisy and, thus, not significant anymore due to the short overlap in years and the consequent loss of data. The sign of the point estimates suggests that 2 years of parental leave improves maternal outpatient health instead of only 1 year. 1.5 years is even better for maternal outpatient health than 2 years of parental leave.

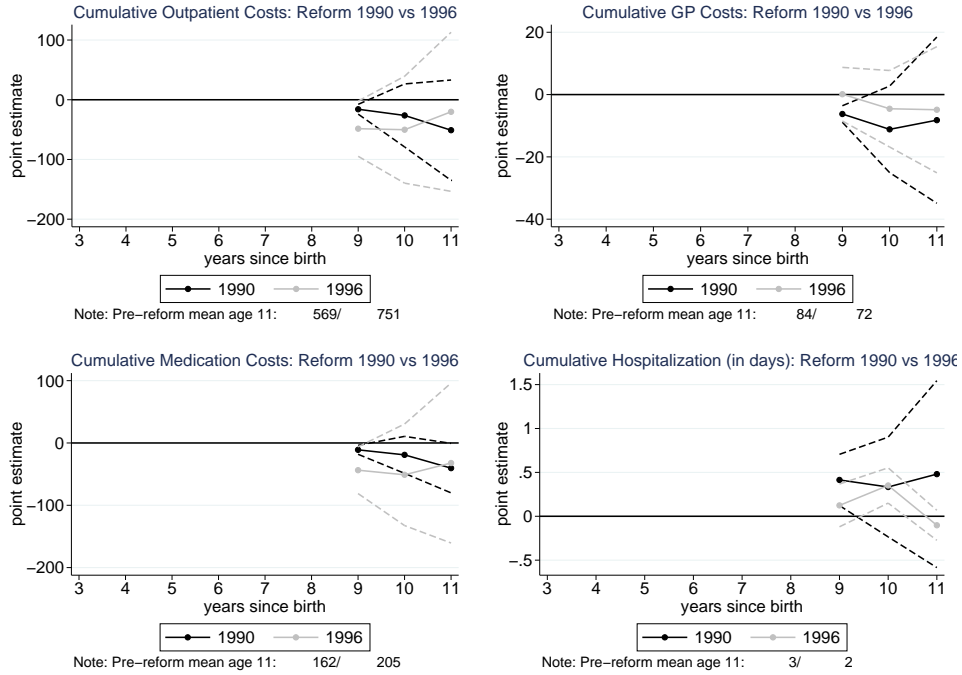
Panel B, which compares the 1996 and 2000 reforms, complements this picture. While the two outcome measures of outpatient costs and medication costs clearly improve with the 1996 reform, the same measures substantially deteriorate with the increase of parental leave of 1 year to 2.5 years in 2000. This effect pattern becomes even larger over time when moving toward 7 years after giving birth.

This analysis adds to the existing literature in the following dimension. As I can compare several reforms in the same setting, and as these reforms both extend and decrease parental leave, I can say something about the health gradient. This study is the first to provide evidence that returns to longer leave can even become negative with too long parental leave. This suggests a hump-shaped relationship between parental leave duration and maternal health in the medium- to long-run.

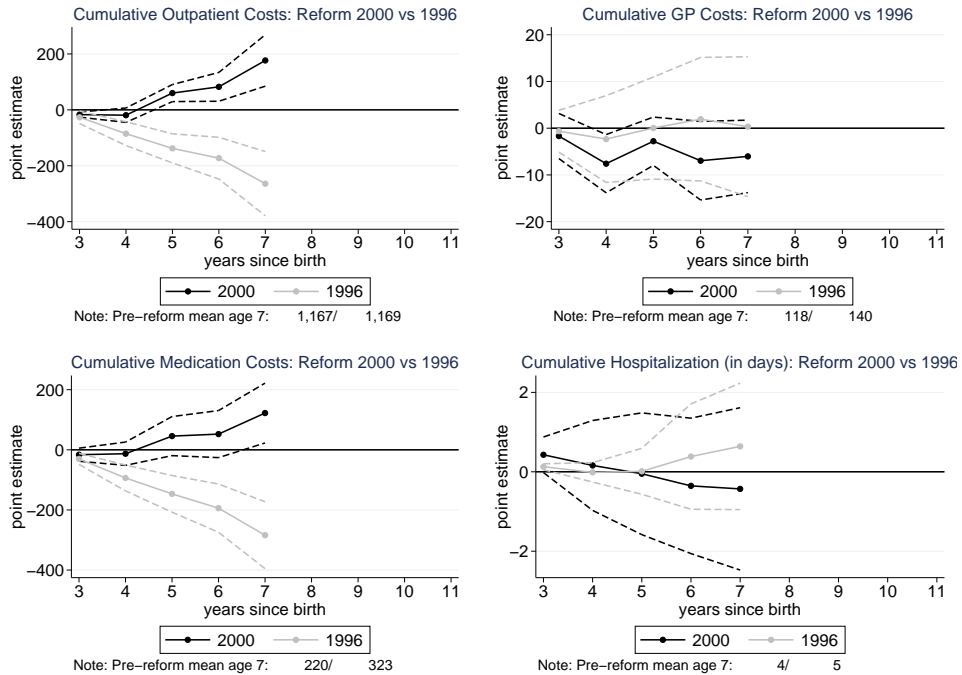


Figure 1.3: Comparing the Reforms Directly

### Panel A: 1990 Reform vs 1996 Reform



### Panel B: 1996 Reform vs 2000 Reform



*Notes:* This Figure shows point estimates from estimating Equation (1.1) on different accumulated health outcomes. Each dot represents a separate estimation on accumulating the health outcome up to this point in time. Dashed lines report 95 percent confidence intervals. In Panel A (B) point estimates for the 1990 (2000) reform are reported in black and thus for the 1996 reform in grey.

## 1.5.6 Subsequent Reforms

There were several additional family policy reforms in the 2000s, as described earlier on and summarized in Table 1.1. Interestingly, these reforms allowed parents to choose from a variety of options. As such, mothers who gave birth after the 1st of January 2008, were given two shorter options (20 months and 15 months) additionally to the prevailing 30 months of parental leave. In January 2010, this choice set was enlarged with two other even shorter options. These options would pay benefits for 12 months, either for a fixed and flat benefit amount or for 80 percent of the last income.<sup>11</sup> In general, shorter leave duration pays a higher monthly benefit. However, in total, longer leave always sums up to higher benefits.

None of these reforms can be evaluated with respect to the health outcomes, as I only observe maternal health up to the year 2007. Nevertheless, it is interesting to look at the choices of the mothers by income.<sup>12</sup> These choices are presented in Table 1.9.

In 2007, when the parental leave is designed for 30 months, most of the mothers are choosing this long version. Mothers can choose to finish parental leave earlier, which more high-wage mothers than low-wage mothers choose to do. In 2008 and 2009, when the two shorter options of 15 and 20 months become available, a substantial fraction of mothers switch to one of these options. Roughly 15 percent of low-wage mothers and 20 percent of high-wage mothers switch, respectively. As a result 70 percent of low-wage mothers are still taking the longest available option, and 56 percent of high-wage mothers stick to it.

In 2010, where 12 months of leave are added to the choice set, the fraction of high-wage mothers choosing this option increases drastically to 18 percent. This is the result of the possibility of being paid 80 percent of the last wage, which attracts especially high-wage

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<sup>11</sup>This last option was designed for fathers, to encourage paternal uptake of parental leave.

<sup>12</sup>This is the only characteristic that is still available for these mothers, as the availability of the birth register ends in 2007.

mothers. In 2010, 64 percent of low-wage mothers are on leave for 21–30 months and 46 percent of high-wage mothers are on leave for 21–30 months.

Altogether, as very long leave (2.5 years) has been shown to be harmful for maternal health for all types of mothers, the percentages shown in Table 1.9 may suggest that a broad range of options could increase inequality in this setting. High-wage mothers are better in optimally choosing the months of parental leave. The structure of the parental leave system also incentivizes them to go for shorter leave. This could be corrected by allowing for different parental leave lengths with higher benefits per month for shorter leave duration but an overall total amount that is the same among all options. The current system, instead, favors long leave duration for low-wage mothers.<sup>13</sup>

## 1.6 Conclusion

I exploit three reforms that changed the length of parental leave to estimate the effect on maternal health in the short- to long-run. The first reform in 1990, increased parental leave by 1 year to 2 years. The second reform in 1996, partially reversed this back again to 1.5 years. Finally, the third reform in 2000 increased parental leave to 2.5 years, thus increasing it again by 1 year. There was strict enforcement, so that mothers giving birth on June 30 would be subject to the old policy regime and mothers giving birth on July 1 to the new one. This allows to implement a regression discontinuity design, where I additionally control for seasonality by estimating it in differences to the preceding non-reform year. Maternal health can be evaluated from 1998 to 2007, and thus allows me to estimate the impact of the policy reform from 1 year pre-birth up to 17 years post-birth.

The results provide evidence for a hump-shaped relationship between maternal health and parental leave duration. Especially the 2000 reform seems harmful to maternal health

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<sup>13</sup>The total amount paid to a mother who goes on leave for 12 months is €12,000. The total amount paid to a mother who goes on leave for 30 months sums up to €13,080.

Table 1.9: Parental Leave Decisions of Mothers from 2007 to 2010 by Income

	Low-wage	High-wage
<b>Choices in 2007</b>		
1-12m	4.81	5.64
13-15m	2.25	3.95
16-20m	7.57	14.12
21-30m	85.36	76.29
Observations	21,813	23,031
<b>Choices in 2008</b>		
1-12m	6.02	6.83
13-15m	3.62	5.71
16-20m	17.43	26.44
21-30m	72.93	61.01
Observations	23,741	25,099
<b>Choices in 2009</b>		
1-12m	6.62	9.18
13-15m	3.85	7.76
16-20m	19.37	26.77
21-30m	70.15	56.30
Observations	23,742	25,557
<b>Choices in 2010</b>		
1-12m	10.82	18.00
13-15m	5.38	12.69
16-20m	19.37	23.31
21-30m	64.43	46.00
Observations	25,727	27,893

*Notes:* This Table shows the percentage of eligible women who select into 1–12/13–15/16–20/21–30 months of parental leave by wage-type for the years 2007–2010. The month-ranges are derived from the set of options that are available to mothers as pointed out in Table 1.1. While in 2007 parental leave is designed for 30 months, additional options become available in 2008 and 2010.

measured in the outpatient sector. Nevertheless, there seems to be a trade-off between worse outpatient health and better inpatient health. Zooming into maternal health, the drivers for the observed pattern seem to be prescribed medication costs. Of these, general nervous system medication and more specifically painkillers are significant explanatory variables. For all three reforms, effects accumulate over time, such that one would expect even stronger effects in the very long-run.

A heterogeneity analysis reveals interesting additional insights. Low-SES mothers (proxied by low-wage and unmarried) benefit more from the first extension to 2 years. While the second reform that decreases parental leave to 1.5 years, generally has no significant impact on outpatient health, this is not true for mothers with unhealthy babies (proxied by a preterm birth or low birth weight). For mothers of unhealthy babies a reduction in parental leave to 1.5 years increases their health costs significantly. The 2000 reform, finally, is especially harmful for mothers who already showed mental disorders pre-birth.

A descriptive analysis of subsequent reforms concerning the family policy system, which enable mothers to choose from a variety of options, reveals that high-wage mothers are more likely to choose shorter durations than low-wage mothers. This might suggest that, as very long leave is harmful for all types of mothers, the current policy structure could increase health inequalities across low- and high-wage mothers.

This paper confirms the findings of Butikofer et al. (2018) who state that there are diminishing returns to maternity leave length. Cautiously interpreting the findings in this paper, one could possibly argue that the returns can even become negative with too long parental leave length. The heterogeneity analysis also shows that it is very important to take birth outcomes, such as the baby being born preterm or with low birth weight, and maternal characteristics into account as well, when designing family policies.



# Chapter 2

## Womb at Work:

## The Missing Impact of Maternal Employment on Newborn Health

*A version of this paper is under review at the Journal of Health Economics.*

### 2.1 Introduction

Most high-income countries have seen a significant and steady increase in female labor force participation over the last few decades. Therefore, women today are much more likely to work while pregnant. At the same time, family policies have become much more generous since the turn of the century with multifold goals such as gender equity, higher fertility, and better child development (Olivetti and Petrongolo, 2017). One family policy instrument is prenatal maternity leave intended to protect both the health of the mother and the newborn. The duration of prenatal leave varies substantially—from 0 to 11 weeks—in European countries (Jurviste et al., 2016). This variation across countries mirrors the uncertainty among policy makers on how to optimally design such programs

and concerning the role of maternal leave. In the fetal origins hypothesis literature, several pregnancy conditions have already been identified as key influencing factors on a variety of long-term outcomes of children. If prenatal maternal employment is among them, long-term benefits for the children may offset the costs of prenatal maternity leave. Therefore, understanding the effects of maternal employment during pregnancy on newborn health is key for policy makers who design policies concerning prenatal maternity leave.

There is a large body of literature estimating the effects of pregnancy conditions on newborn health and long-term outcomes of children as summarized by Almond and Currie (2011) and Almond et al. (2018). However, there is little evidence on the effect of prenatal employment on newborn health. This is critical because the prenatal maternal employment status combines several major aspects of pregnancy conditions such as stress, physical activity, disease environment, income, and others. In theory, the impact of prenatal employment is ambiguous. On the one hand, maternal employment during pregnancy may be stressful for the mother or may correlate with exposure to pollutants and diseases. These influences have been shown to be detrimental to the unborn baby (Aizer et al., 2015; Currie and Schwandt, 2013, 2016; Schwandt, 2018). On the other hand, employment may also increase a mother's income or may be a joyful activity itself, which therefore could improve newborn health (Almond et al., 2011; Hoynes et al., 2015).

This paper provides evidence on the effect of maternal employment during pregnancy on newborn health by exploiting three reforms in Austria that affected mothers' likelihood of working during pregnancy with their second child. These three reforms on the duration of parental leave allow me to employ a regression discontinuity setting. In order to empirically analyze the impact of prenatal maternal employment, I use administrative data from the Austrian Social Security Database (ASSD), which contains the full work history for private-sector employees. This data set can be linked to the Austrian Birth



Register (ABR), which covers all births with several indicators on newborn health and characteristics about mothers.

Parental leave policies in Austria consist of both a flat benefit and job protection. Beginning in 1990, there have been several reforms affecting the duration of parental leave. In 1990, parental leave was extended by one year, from 12 to 24 months. In 1996, this was partially reversed to 18 months, but increased to 30 months in 2000. These changes led to an easier automatic extension for another parental leave period upon the birth of an additional child—the so called *grace period* rule. This rule exempts mothers from working and therefore reduces a mother’s probability of working during pregnancy with the second child.

I find no evidence of prenatal employment effects on newborn health. This holds true for a variety of outcomes measured via birth weight, gestational age, and Apgar scores. Thus, I cannot reject that there are no effects of maternal employment during pregnancy on newborn health, despite a very strong first stage. Across all policy reforms, the duration of parental leave significantly affects the mother’s employment status during pregnancy with her second child. The effect of the July 1990 reform, which increased parental leave by 12 months to 24 months, corresponds to a 19.1 percentage point decline in the share of mothers’ working during pregnancy. This effect is homogeneous over the entire first seven months of a pregnancy. A heterogeneity analysis reveals marginally different, but always significant, effects for a large set of subsamples stratified by a mother’s marital status, occupational collar, and industry. However, I detected no significant effects on newborn health for any of these subsamples.

Significantly, the study is based on a large administrative sample that allows me to precisely estimate the effects and to rule out sizeable newborn health effect-patterns. Furthermore, all covariates vary smoothly around the cutoffs of the policy reform, which

supports the interpretation of the regression discontinuity; the results are robust to a variety of different specifications. For example, I test for different birth weight and gestational age thresholds, apply a Donut estimation to deal with delayed Caesarean sections, and implement a bounds estimator to control for selection.

This paper makes several contributions to the literature on pregnancy conditions on infant health and specifically, on prenatal maternal employment on infant health.<sup>1</sup> Wüst (2015) employs Danish survey data in a regression analysis and finds positive effects for working mothers with closely spaced consecutive births or those who change their employment status due to educational reasons. Rossin (2011) analyzes the impact of unpaid maternity leave provisions in the United States and documents small increases in birth weight and a reduction in premature birth and infant mortality. Stearns (2015) studies time off from work during late pregnancy under a temporary disability insurance program. She finds beneficial effects for newborns of unmarried and black mothers. Ahammer et al. (2018) analyze a 1975 reform in Austria that extended prenatal maternity leave from six to eight weeks. They find no evidence for significant effects on newborn health.<sup>2</sup>

My paper adds to this literature in the following ways. It is the first to provide clear evidence of the effect of maternal employment up to and including the seventh month of a pregnancy on the health of the newborn. This setting, when combined with pre-

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<sup>1</sup>This study also relates to the literature studying the effects of parental leave on other types of outcomes such as maternal labor market outcomes, fertility, child and maternal health, and cognitive development of children. In the Austrian context, Lalive and Zweimüller (2009) and Lalive et al. (2013), for example, study the effect of the same reforms on maternal labor market outcomes and fertility, while Danzer and Lavy (2018) and Danzer et al. (2017) focus on cognitive outcomes of the affected children. More broadly, there is a large body of literature that studies the effect of post birth parental leave reforms on child development (Baker and Milligan, 2008b; Beuchert et al., 2016; Carneiro et al., 2015; Dahl et al., 2016; Danzer and Lavy, 2018; Dustmann and Schönberg, 2012; Ruhm, 2000; Tanaka, 2005; Rasmussen, 2010). Generally, the literature concludes that introducing parental leave significantly improves child development in both the short and long run while extensions in the duration of parental leave often do not lead to significant changes in child development.

<sup>2</sup>A related study by Ginja et al. (in press) analyzes the effect of a speed premium in Sweden, analogous to the grace period in Austria. In their study, the effect of maternal employment on newborn health is not directly addressed. However, they document a slight decline in maternal employment during pregnancy with the second child and no effects on outcomes measured at birth as a result of the speed premium. The effect size of the analyzed policy reform on maternal employment is -0.013, substantially smaller than the 0.191 I find.

vious literature that focused almost exclusively on the very end of a pregnancy, helps in understanding *when* in pregnancy time off might be most beneficial. Using the described reforms for exogenous variation in prenatal maternal employment up to the 32nd pregnancy week generates a large and representative sample of compliers. This complements the existing literature on maternal employment during pregnancy on newborn health as I do not have to rely on differential take-up of welfare programs by rich and white mothers (as in the context of unpaid leave) or unmarried and black mothers (in the case of disability insurance). Furthermore, I am able to use a large administrative data set on an individual basis that allows me to exactly identify the exposed mothers and their offspring. Based on this data set, I can calculate the exact number of days a mother is working during pregnancy. Significantly, this allows me to analyze the impact of prenatal employment on two margins—the extensive margin of mothers who choose to work or not during pregnancy and the intensive margin of working mothers—as I can factor in unusually detailed information on maternal employment histories. The richness of the data also makes it possible to analyze heterogeneous effects across mothers and by work environment. Finally, in the Austrian context, the rich data can be combined with a diverse policy setting. I explore the impact of prenatal employment in three different points in time that allow me to both assess the effects of increases as well as decreases in employment during pregnancy. Variation in both directions allows for study of asymmetries in an already generous leave setting with strong changes in parental leave duration across the studied time period.

Overall, my results show that large changes in prenatal employment do not imply significant changes of newborn health. This suggests that parental leave policies should focus on parental leave after the birth, given that time spent with parents especially in very early childhood has been shown to be beneficial in a number of outcomes (Carneiro

et al., 2015; Rossin-Slater, 2018; Heckman, 2007).

The paper is organized as follows: Section 2.2 describes the Austrian parental leave system and the reforms used for this study. Section 2.3 develops a conceptual framework on how prenatal maternal employment can affect newborn health. Section 2.4 discusses the data and Section 2.5 presents the empirical strategy. Section 2.6 provides an overall assessment of the results and sensitivity analyses, which will be discussed in Section 2.7. The paper concludes in Section 2.8.

## 2.2 The Institutional Setting

The Austrian family policy rules consist of two types of policies that cover the period around birth. The first policy, mandatory maternity leave (ML), prohibits work 8 weeks pre- and post-birth and pays the average wage a mother earned during the last quarter prior to giving birth. As such, ML promises a generous environment in order to protect both the mother's and the baby's health. After mandatory maternity leave expires, eligible mothers can choose to take parental leave (PL). This second policy consists of two pillars: a flat benefit and job protection. The policy changes that I will assess in this paper affect parental leave, which will be explained in more detail below.

First-time mothers over the age of 25 are eligible for parental leave if they have worked for at least 52 weeks within the 2 years prior to giving birth.<sup>3</sup> The work requirement is reduced to 20 weeks within the year prior to giving birth for higher-order births.<sup>4</sup> Furthermore, there is a *grace period* rule that allows mothers with relatively short birth spacing an automatic extension for the next birth. More specifically, the grace period exempts mothers from the work requirement if they give birth to an additional child no later than 3.5 months after the expiration of the parental leave of the previous child.

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<sup>3</sup>Work requirements are shorter for younger mothers.

<sup>4</sup>This is changed to 26 weeks after July 1996.

Since 1990, there have been several reforms to the parental leave system; they are illustrated in Figure 2.1. Political debate about the introduction of paternal leave at the costs of maternal leave led to the compromise of extending the duration of parental leave. Furthermore, in the early 1990s, formal child care institutions were scarce in Austria. This made maternal employment during the child's early years difficult and could possibly even deter mothers from the labor market in the long run. Extending parental leave was expected to improve this situation for mothers.

The reforms were structured as follows. While parental leave lasted up to the first birthday of the child until June 1990, this was extended to 24 months after birth in July 1990. The July 1996 reform, which targeted the cash benefits but left job protection unchanged, reserved 6 of these added months for fathers. This effectively reduced the duration of parental leave to 18 months after birth because almost no fathers were taking advantage of the leave. The July 2000 reform, which again only targeted cash transfers, increased parental leave to 36 months after birth, reserving 6 months for fathers. Therefore, it essentially increased parental leave from 18 to 30 months after birth.<sup>5</sup>

The changes in parental leave duration had strong implications for the likelihood that mothers would give birth to another child within the grace period. Prior to June 1990, a mother had to conceive the next child no later than 5.5 months after giving birth to her first child in order to meet the requirements for automatic extension, which is biologically difficult.<sup>6</sup> The 1990 reform extended the window to conceive to 17.5 months; the July 1996 reform partially reversed it to 11.5 months, while the July 2000 reform extended it again to 23.5 months. All of these time windows are biologically feasible and desirable.<sup>7</sup>

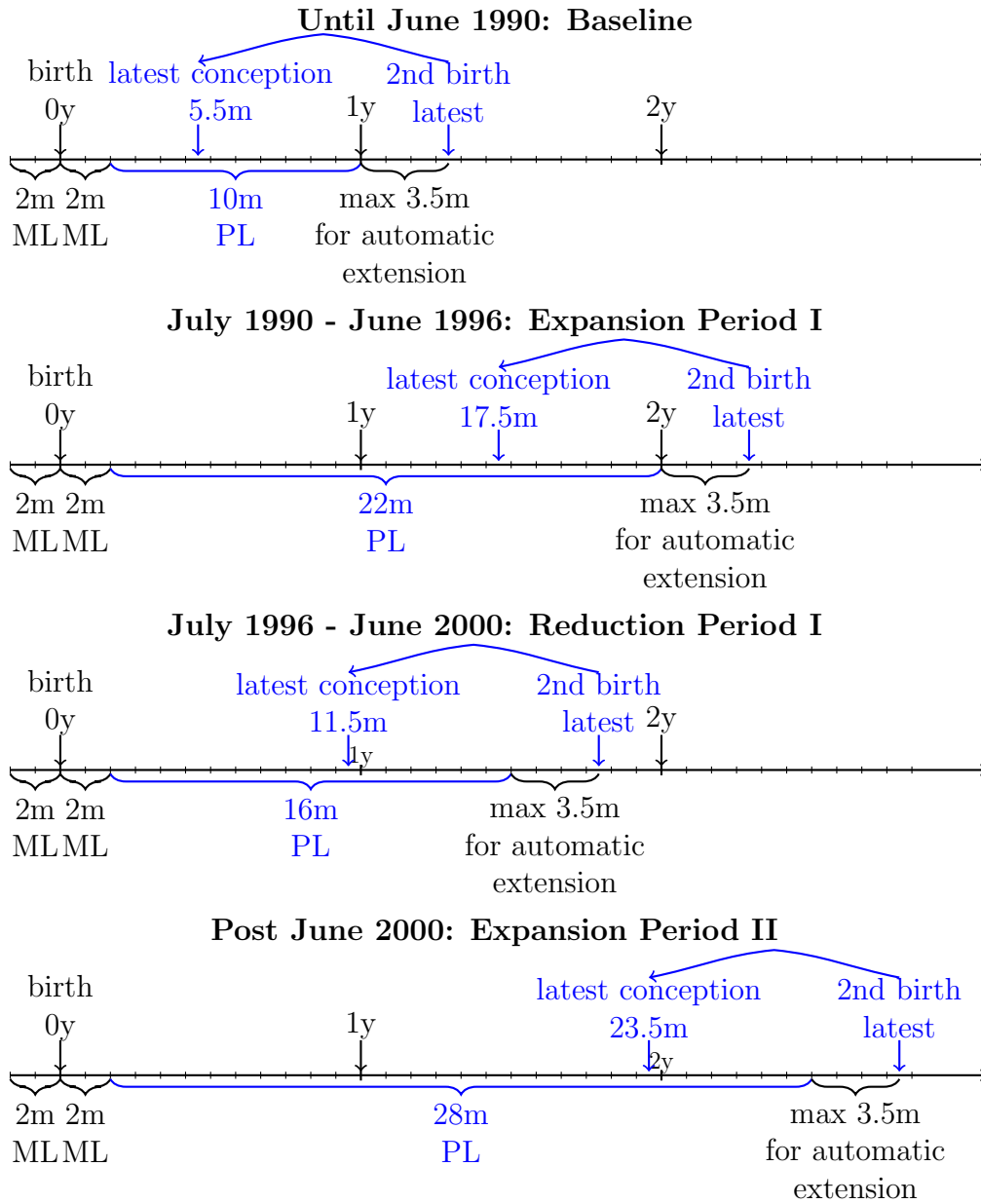
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<sup>5</sup>For further and more detailed information on the amount of cash benefits, eligibility criteria, announcement of the policy reforms, and the reforms themselves, see Lalive and Zweimüller (2009) and Lalive et al. (2013).

<sup>6</sup>Mothers who exclusively breastfeed have 98 percent protection from pregnancy in the first six months (Kennedy et al., 1989).

<sup>7</sup>Short (often defined as less than 18 months) and very long (more than 59 months) interpregnancy intervals are associated with adverse perinatal outcomes (see Conde-Agudelo et al. (2006) for a meta-analysis on birth spacing).

Figure 2.1: Overview Policy Changes



*Notes:* This Figure shows in an illustrative way how the three policy reforms affected the latest conception date for a second birth so that mothers would automatically be able to prolong their first parental leave spell without having to go back to work during pregnancy with the second child. Parental leave solely refers here to the provision of cash benefits. While job protection increases in the first reform to 24 months it stays unchanged thereafter.

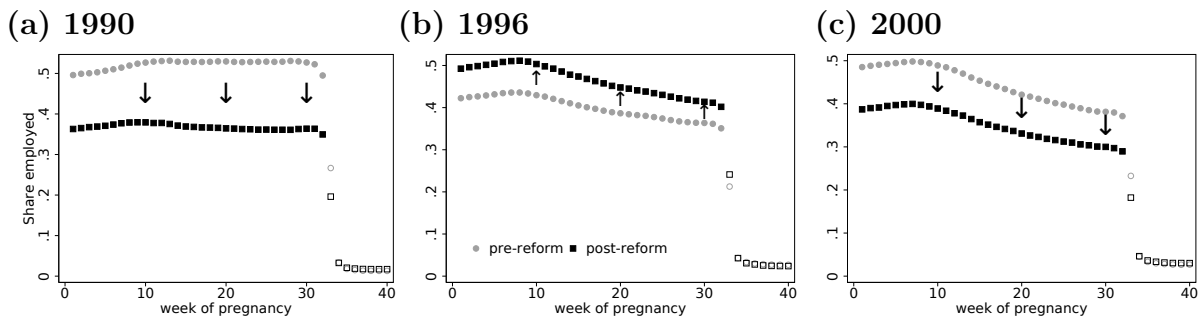
Figure B.1 in the Appendix<sup>8</sup> shows how the percentage of children born within parental leave and the grace period has evolved over time. By construction, longer parental leave will lead to higher fractions of second children born within this time period. Although prior to July 1990, only 10 percent of all second born were born within the period of

<sup>8</sup>All following Figures and Tables denoted by B are reported in the *Appendix: Chapter 2*.

parental leave extended by the grace period, that number increased to around 40 percent for the period from July 1990 to June 1996. In July 1996, there was a drop to 20 percent given the shorter parental leave for these cohorts. The number increased again with the very generous parental leave system established in July 2000.

Figure 2.2 exploits the discontinuities induced by the reforms and looks at pregnancies with second children for mothers that gave birth to their first child each of the 2 years pre- and post-policy reform. It shows that this simplification in meeting the requirements for the grace period in July 1990 went hand in hand with fewer women working during pregnancy with their second child. These effects are equally spread over the entire first 32 weeks of pregnancy. Thereafter, mothers go on mandatory maternity leave and do not have to work under either scheme. The decline in duration in July 1996 led to more women working during the entire pregnancy, while the increase in duration in July 2000 again translated into fewer women working during pregnancy with their second child.

Figure 2.2: Employment Status of Mothers by Week of Pregnancy



*Notes:* This Figure reports the share of women who work during pregnancy with their second child. Mothers considered are all women pregnant with their second child who gave birth to their first child 24 months before and after the policy changes in July 1990, July 1996 and July 2000. Hollow squares and circles refer to weeks where mothers are supposed to be on maternity leave (after week 32). The sample consists of all matched and eligible mothers that are still pregnant at a given week of pregnancy (i.e. with preterm births the sample gets smaller moving from week 0 to week 40).

## 2.3 Conceptual Framework

Prenatal maternal employment may affect newborn health through various channels. While several of these channels have each been analyzed individually in the previous literature, the combined effect of prenatal employment on newborn health is not clear a priori. I next describe each of these possible channels in more detail.

First, working itself could be stressful for the mother. Existing empirical literature on the effects of maternal stress on infant health is generally limited to studying the effects of very severe but often unique stress factors such as domestic violence (Aizer, 2011), hurricanes (Currie and Rossin-Slater, 2013), political uprising (Lee, 2014), death of a relative (Black et al., 2016; Persson and Rossin-Slater, 2018), and terrorism (Quintana-Domeque and Ródenas-Serrano, 2017; Camacho, 2008). Although all these studies show a negative impact of these events on newborn health, the effects of milder stress, for example, stress induced by work, are less clear. As recently summarized by Almond et al. (2018), relatively mild shocks in early or prenatal life can, however, have substantial negative impact on child development. Additionally, it is unclear whether mental and physical stress affect the fetus similarly and whether these stresses might cause different effects when they are experienced early or late in pregnancy.

Second, being on PL and therefore not working during pregnancy is also related to changes in income. The intensity of this effect depends on a mother's income. In fact, PL benefits are flat over the entire PL and amount to roughly €340 per month.<sup>9</sup> This translates into an income increase for low-income mothers and an income reduction for high-income mothers. However, in the literature, the income effect on newborn health is almost exclusively identified for low-income mothers. Hoynes et al. (2015), for example, show that the Earned Income Tax Credit (EITC) in the United States reduces the inci-

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<sup>9</sup>According to Lalive and Zweimüller (2009) this corresponds to a median net income replacement ratio of more than 40 percent.



dence of low birth weight and increases mean birth weight. Almond et al. (2011) exploit monthly variation in the introduction of the Food Stamp Program (FSP) in the United States and find positive effects for birth outcomes especially at the lower tail of the birth weight distribution. Both papers focus on programs designed for low-income people. Furthermore, evidence from conditional cash transfers in developing countries also show a positive impact on several birth outcomes (Barber and Gertler, 2008; Amarante et al., 2016). However, as PL benefits are flat and therefore only negatively impact high-wage mothers, the effect of income in this setup is less clear. I will investigate this issue in a heterogeneity analysis.

Third, working during pregnancy might expose the mother to pollutants and diseases at work or while commuting. These influences have been shown to be negative for the fetus (Almond, 2006; Schwandt, 2018; Currie et al., 2009; Currie and Schwandt, 2013, 2016). Both Almond (2006) and Schwandt (2018) even show that flu exposure during pregnancy has long-term effects on children’s educational attainment and wages.

Fourth, a side effect of the extension of the grace period could also be shorter birth spacing between siblings. The medical literature argues that short spacing (most often defined as less than 18 months in age difference) leads to adverse infant health outcomes (see Conde-Agudelo et al. (2006) for a meta-analysis).

## **2.4 Data**

This project is based on two administrative data sets. The primary source of data for the determination of a mother’s work status during pregnancy is the Austrian Social Security Database (ASSD). For the analysis of newborn health outcomes, I link the ASSD to the Austrian Birth Register (ABR).

*The Austrian Social Security Database.* The ASSD stores the full work history of private-sector employees and is used to verify pension claims. The ASSD also records the date of all live births after entering the labor market and maternity and parental leave periods if taken. As a result, I observe detailed information on a daily basis for each woman after her first entry into the labor market. Detailed labor market information consists of the employer, along with information on occupation, experience, and tenure. Information on earnings is provided per year and per employer.

*The Austrian Birth Register.* Information about newborn health measures is based on the ABR, which includes all live births in Austria. Each birth entry consists of individual-level information on birth characteristics such as date, place, gender, multiple births, gestational length, birth weight, length, and Apgar scores. Furthermore, for every birth, maternal socioeconomic characteristics such as age, educational attainment, marital status, and country of origin complement the individual-level information.

I match the two sources of data based on characteristics I observe in both data sets, such as the date of birth, the date of preceding birth for higher-order births, location of birth, and age of the mother.<sup>10</sup> For my main analysis, I restrict my sample to private sector dependent employees who are PL eligible, aged 15–45, and who are giving birth to a singleton.<sup>11</sup> Furthermore, I restrict my main sample to the period of 1984 to 2007, as gestational length, one of my key variables, is only recorded after 1984. Altogether, this

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<sup>10</sup>This results in 61 percent of matches of all births in the birth register, corresponding to roughly 80 percent of all births observed in the ASSD. Once a mother leaves the labor market and gives birth to additional children, these birth dates will not be recorded in the ASSD any longer. However, based on information about the characteristics of the mother herself and the date of birth of the preceding child, I am able to recover some births that are only observed in the Austrian Birth Register. The mother's unique social security number can be added to these recovered births. This method allows me to add 87,362 births to the combined data set.

<sup>11</sup>For PL eligibility I follow the definition of Lalive and Zweimüller (2009). In order to construct the eligible sample, I consider the work history 2 years prior to giving birth to the first child. Women who show any form of employment or were eligible to collect unemployment benefits are considered eligible. This very generous form of eligibility results in 95 percent of women being eligible for PL.

results in around 60,000 observed births per year out of roughly 85,000 births in Austria.

Finally, I construct several variables that are key for my analysis. From all birth entries per mother, I calculate the parity for every birth. Furthermore, I calculate the days a mother has been working during pregnancy and based on this, I create a dummy for the work status, defined as 1 if she worked a positive number of days during pregnancy.<sup>12</sup>

In addition to days worked, days sick during pregnancy can be calculated in the same manner. From this, I create a sickness dummy equaling 1 if a positive number of days sick are reported. This sickness dummy will be used as an additional explanatory variable in the OLS regression and allows me to compare my results to Wüst (2015).

For the analysis of the impact on newborn health, I use several measures of birth outcomes because these have already been shown to be linked to later-life outcomes (Almond and Currie, 2011). First, I include a dummy for low birth weight (below 2,500 grams) as a general measure. Second, I construct a dummy for prematurity equaling 1 if gestational length is less than 37 weeks.<sup>13</sup> Additionally, I will show results for birth weight, gestational length, 1 minute Apgar score, and a dummy for a healthy Apgar score ( $1[\text{Apgar} \geq 7]$ ). The Apgar score is a method of giving a quick summary of the newborn's physical health. It individually measures per category, on a scale from 0 to 2, the newborn's skin color, heart rate, reflexes, muscle tone, and respiratory effort. A total Apgar score of 10 indicates perfect health, and an Apgar score below 7 is defined as low.

Finally, I control for the following maternal characteristics: a dummy for marital status, an indicator of foreign origin, 5-year age brackets, and an indicator combining wage

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<sup>12</sup>I count both normal as well as marginal employment as working days and use the terms worked and employed interchangeably. For the construction of the number of days a mother worked, gestational length in weeks (multiplied by 7 days a week) is subtracted from the exact date of birth. Working days (MO-FR) are being calculated from the day of conception to the last day of pregnancy.

<sup>13</sup>Both thresholds of weight (low birth weight) and gestational age (preterm) are defined by the World Health Organization.

and educational data.<sup>14</sup> For the heterogeneity analysis, I further add a dummy for having worked in a blue-collar job during the first pregnancy.

*Summary Statistics.* Table 2.1 presents the descriptive statistics for the full matched and eligible sample. While column (1) describes the full sample, columns (2)–(4) restrict the sample to second-born children with older siblings born in the vicinity of the policy reform thresholds.<sup>15</sup> When compared to the full sample, second born with older siblings born around a policy reform are less likely to be born preterm and with low birth weight. Their mothers are more likely to be married and older, as expected. Therefore, as I will focus on higher-order born children who are potentially affected by their mother’s longer (or shorter) parental leave stay with the first-born child, the sample will be slightly positively selected with respect to newborn health. However, as 78 percent of Austrian families have more than one child in my sample, this implies that my analysis is based on a non-negligible fraction of births.

In the restricted samples (columns (2)–(4)), the average birth weight ranges from 3,381–3,419 grams and the average gestational length is around 39.7 weeks. The average occurrence of a low birth weight birth is between 3.2–3.3 percent and the one of a preterm birth 3.2–3.6 percent. The 1 minute Apgar scores vary between 8.7–8.9 and 95.8–97.5 percent of the newborns have a healthy Apgar score above 7. Most of the mothers are married (0.72–0.80) and give birth to their second child between the ages of 25–29 (0.38–0.45). On average, about 60 percent of the mothers work during pregnancy with their second child and spend 63–69 days at work.

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<sup>14</sup>Following Danzer et al. (2017), I construct this indicator variable by defining all mothers as low SES who completed compulsory schooling or who completed apprenticeship training or intermediate vocational school and additionally have low wages. For the assignment to high and low wage, I use average wage data in the two years prior to a mother’s first birth. I classify a women as low wage if her wage is below or equal to the median wage in that specific birth year for all women in my sample. Mothers who have completed at least high school or who finished apprenticeship training or intermediate vocational school and earn a high wage are defined as high-SES mothers.

<sup>15</sup>The chosen bandwidth in my baseline analysis is 24 months pre- and post-policy reform.

Table 2.1: Descriptive Statistics

	Full sample	1990 sample	1996 sample	2000 sample
Birth weight (in gram)	3,320.028 (519.768)	3,381.319 (500.795)	3,410.534 (505.488)	3,419.033 (503.999)
Birth length (in cm)	50.494 (2.641)	50.699 (2.507)	50.785 (2.559)	50.746 (2.572)
Gestational length (in weeks)	39.686 (1.776)	39.797 (1.625)	39.757 (1.678)	39.665 (1.710)
Preterm birth	0.043	0.032	0.033	0.036
Low birth weight	0.046	0.032	0.032	0.033
Apgar 1min score	8.741 (1.096)	8.798 (0.992)	8.850 (0.950)	8.891 (0.903)
Healthy Apgar score ( $\geq 7$ )	0.958	0.968	0.971	0.975
Female baby	0.487	0.489	0.486	0.487
Mother married	0.688	0.804	0.763	0.724
Mother foreign	0.121	0.087	0.140	0.152
Mother aged 15-19	0.056	0.013	0.009	0.009
Mother aged 20-24	0.278	0.248	0.174	0.178
Mother aged 25-29	0.356	0.445	0.412	0.375
Mother aged 30-34	0.221	0.230	0.312	0.323
Mother aged 35-39	0.077	0.058	0.086	0.103
Mother aged 40-45	0.012	0.006	0.008	0.011
Mother of low SES	0.519	0.529	0.464	0.436
Worked	0.759	0.611	0.607	0.563
Days worked	95.881	69.826	68.847	63.528
Observations	1,279,374	87,566	77,279	63,481

*Notes:* The full sample covers the universe of births occurring from 1984 to 2007 to matched and eligible mothers. Columns (2)–(4) restrict the full sample to second born children with an older sibling that is born 24 months pre- and post a policy reform. The policy reforms considered happen in July 1990 for column (2), July 1996 for column (3) and July 2000 for column (4).

## 2.5 Empirical Design

This paper focuses on identifying the causal effect of working during pregnancy on newborn health. Specifically, consider the following baseline model:

$$Y_i = \beta_0 + \beta_1 work_i^m + \beta_2 X_i^c + \beta_3 X_i^m + \tau_t + \epsilon_i, \quad (2.1)$$

where for each individual  $i$ ,  $Y_i$  is the outcome of interest.  $work_i^m$  represents a dummy for the work status of the mother during pregnancy or the days worked.  $X_i^c$  controls for the

child's gender. The vector  $X_i^m$  of maternal characteristics includes controls for 5-year age brackets, marital status, origin, and low SES.  $\tau_t$  is a vector controlling for year of birth and month of birth fixed effects and  $\epsilon_i$  is an error term.  $\beta_1$  is the coefficient of interest corresponding to the effect of prenatal employment on newborn health.

One issue with estimating Equation (2.1) is omitted variable bias. One such example is the mother's own health status. A mother might choose not to work because she is in bad health, which also directly influences the health of her own child. Other unobserved variables that could threaten the validity of a standard ordinary least squares (OLS) approach, are, for example, income and stress.

*Regression Discontinuity Design (RDD).* To overcome the issue of omitted variable bias, I base my analysis on the described reforms of the parental leave system that generate quasi-experimental variation in the likelihood of mothers working during pregnancy. More specifically, I use three separate regression discontinuities for the three reforms in July 1990, July 1996, and July 2000. The RDD method and its use in economics is extensively summarized by Imbens and Lemieux (2008) and Lee and Lemieux (2010b). It is based on the intuition that all mothers giving birth to their first child just before or just after a policy change do not differ discontinuously in their characteristics, but rather, face two different policies that discontinuously affect a mother's prenatal employment status.

To see this better, imagine two mothers, Mother A and Mother B. Mother A gives birth to her first child in June 1990 and can stay on parental leave up to the first birthday of her child. She has to conceive her second baby by December 1990. If she meets this time window, she does not have to go back to work during the pregnancy with her second child. Mother B gives birth to her first child in July 1990, only one month later than Mother A. She can stay on parental leave for two years and must conceive by January

1992 in order to be exempt from the work requirement. Mother A and Mother B are unlikely to differ in characteristics ex ante. However, Mother B is much more likely to give birth to another child within the grace period. This discontinuity in the likelihood of working during pregnancy with the consecutive child stemming from an exogenous policy reform will be exploited in the following estimation.

*Estimation.* Following Jacob et al. (2012) and Lee and Lemieux (2010b), I estimate local linear regressions in samples around the cutoffs. I estimate separate OLS regressions with a rectangular kernel for each cutoff date and choose in my context a meaningful bandwidth of 24 months. Following Lee and Lemieux (2010b), the choice of kernel typically has little impact in practice. I also conduct a sensitivity analysis with different bandwidths chosen by the method of Imbens and Kalyanaraman (2012) or Calonico et al. (2018).<sup>16</sup> I report graphical evidence of the robustness to different bandwidths in the Appendix.

I start by documenting the discontinuity of prolonged parental leave on prenatal employment for mothers having a second child. I estimate the following first-stage regression:

$$work_i^m = \gamma_0 + \gamma_1 T_i + \gamma_2 R_i + \gamma_3 T_i * R_i + v_i. \quad (2.2)$$

The variable  $T_i$  describes an indicator variable for having an older sibling being born in the post-policy reform period.  $R_i$  is the rating variable, which indicates the number of months from the older sibling's birth date to the date of policy change, and  $v_i$  is an idiosyncratic error term. The parameter of interest in Equation (2.2) is  $\gamma_1$ .  $\gamma_1$  describes

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<sup>16</sup>The bandwidth selection according to Imbens and Kalyanaraman (2012) is implemented in Stata by the command `rdcv` and choosing `ik` as method. The bandwidth selection according to Calonico et al. (2018) is implemented in Stata by the command `rdbwselect` and choosing the default method `mserd`.

the size of the change in the outcome  $work_i^m$  at the date of the policy reforms and therefore highlights the discontinuity in the share of mothers working during pregnancy with their second child.

In a second step, I examine the effect of the policy reform on newborn health using the following reduced form equation:

$$Y_i = \delta_0 + \delta_1 T_i + \delta_2 R_i + \delta_3 T_i * R_i + \nu_i. \quad (2.3)$$

This reduced form equation provides estimates of the net effect of parental leave reforms on newborn health.

*Identification.* The identifying assumptions for inference using the RDD are: (1) the probability of being treated must be discontinuous at the cutoff, and (2) there should be no discontinuity in potential outcomes at the cutoff (Lee and Lemieux, 2010b). The second statement requires that no observable nor unobservable factors exhibit any discontinuities at the cutoffs. This is likely fulfilled if there is no precise manipulation on either side of the cutoff.

While assumption (1) holds by construction of the policy reform, assumption (2) cannot be directly validated. However, I can conduct standard tests for asserting the validity of assumption (2). The timing of the policy announcement guarantees that individuals cannot perfectly sort at the policy reform threshold. As described by Lalive and Zweimüller (2009), the policy reform in 1990, for example, was only announced three months before implementation and therefore made perfect birth planning impossible. However, pregnant mothers could still influence the timing of a birth via a Caesarean section within a short time window, which I will address in the robustness analysis.

Additionally, to assess credibility of assumption (2) based on observables, I visualize



the relationship between the covariates and the rating variable as reported in Figure B.2. In the context of possible confounders, these variables should evolve smoothly across the cutoff. Furthermore, I also report a Covariate Balance Test (see Table B.1), where I test for discontinuities in my observable variables using regressions outcomes. Across all these tests, the smoothness of predetermined observable maternal characteristics is fulfilled.<sup>17</sup>

*Sample Selection.* Although the regression discontinuity design likely circumvents the problem of omitted variables bias described above, it does not solve the problem of sample selection. Sample selection in this context can arise as only newborn health measures become observable for those women who choose to become mothers. If the policy changes regarding the duration of paid parental leave directly affect fertility decisions, this might influence the sample of mothers for which the newborn’s health is observed. In their analysis, Lalive and Zweimüller (2009) and Lalive et al. (2013) show direct fertility effects of the 1990 policy reform, but no such effects for the other two reforms, in 1996 and 2000.<sup>18</sup> In order to address the issue of sample selection in the 1990 reform, I follow the approach described by Kim (2016) and Dong (2019) in a robustness analysis to support the causal interpretation of prenatal employment and not only the net effect of the policy reform.

The approach is based on estimating treatment bounds in the presence of sample selection, leaving the formal specification in Equations (2.2) and (2.3) unchanged. Compared to other approaches that deal with sample selection, the chosen one does not require specifying any selection mechanism nor any exclusion restrictions. The only additional assumption for identification is (3) monotonicity, which implies that observability of out-

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<sup>17</sup>The covariate balance test for the effect on being married reports a significant reduction on being married after the 2000 reform. However, in terms of magnitude this effect is relatively small.

<sup>18</sup>I re-estimate the fertility effect for the 1990 reform with my own sample and find that the policy leads to 2.5 additional children per 100 women within three years. With an estimated first-stage effect size of 19.1 women per 100 women who additionally do not work after the change in the policy, the effect of the sample selection seems relatively small.

comes is only affected in one direction due to treatment assignment. In the described context, this means that all mothers who gave birth to an additional child in the less-generous parental leave period (before July 1990) would also give birth under the new rules. After the policy reform, additional mothers join the sample who are only induced to give birth to another child under the more generous policy scheme. Monotonicity would be violated if there are mothers who would only give birth to an additional child in the less-generous pre-reform policy period. Here, the assumption of monotonicity is reasonable as mothers who give birth after the policy reform could still go back to work after 1 year and face the same opportunities as pre-policy reform.

The treatment bounds are intended to estimate the share of additional mothers in the after-reform sample ( $\sim 5$  percent). These are the marginal mothers who were induced to have a second child by the reform. Their newborns' outcomes are observable after the reform but not before. In terms of outcomes, these newborns cannot be distinguished from the control group. Therefore, without invoking any additional assumptions, one can assume the extreme situation. In this extreme situation, I restrict the sample to a very favorable group (excluding the lower 5 percent in the respective outcome distribution) and a very unfavourable group (excluding the upper 5 percent in the respective outcome distribution). This provides me with a lower and an upper bound.<sup>19</sup>

## 2.6 Results

This section starts by discussing the OLS effects of working during pregnancy on newborn health. I will then show RDD estimates of the effects of changing PL duration on maternal employment during pregnancy with her second child and newborn health. I end by reporting several sensitivity analyses in order to test for robustness.

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<sup>19</sup>Standard errors in this approach are calculated via bootstrapping.

### 2.6.1 Baseline OLS Estimates

Table 2.2 presents baseline OLS results for estimates of Equation (2.1). All columns are estimated on the pooled sample of the three RDD 24-months-bandwidth samples with second-born children. Columns (1) and (3) include only child-level characteristics such as a gender dummy, year, and month of birth fixed effects. In columns (2) and (4), referred to as the full control model, I additionally control for mother-level characteristics: 5-year age dummies,<sup>20</sup> an indicator of foreign origin, a dummy for the marital status, and a dummy for low SES.

Table 2.2 is split into Panel A, the extensive margin effect of the work status on newborn health, and Panel B, the intensive margin effect of days worked on newborn health. Both a mother's work status and the days worked during pregnancy are positively related to newborn health outcome measures. The full control model suggests that children of mothers who work during pregnancy are, at the mean, 6.3 percent less likely to be born preterm and 7.0 percent less likely to be of low birth weight, respectively.<sup>21</sup> The effect sizes are slightly bigger in the full control model for the effect of days worked. A baby of a mother who works 65 days (the average of the three RDD samples) versus that of a mother who does not work at all, is on average 19.4 percent less likely to be born preterm and 19.9 percent less likely to be of low birth weight, all else being equal. Estimates on the two continuous outcome variables of birth weight and gestational length and those for the 1 minute Apgar score and the healthy Apgar score dummy are reported in Table 2.3. They all show the same direction of correlations, while the ones with the Apgar scores are relatively weak.

The sickness dummy is negatively correlated with all newborn health measures, as

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<sup>20</sup>I also test for other specifications of the age variable commonly used in the literature, such as age and age squared. My main coefficients of interest are, however, unaffected.

<sup>21</sup>The percent terms are evaluated at the mean of the dependent variable, i.e. for the outcome variable preterm:  $(0.0335 - 0.0021) / 0.0335 - 1 = -0.063$ .

Table 2.2: OLS Results on Preterm and Low Birth Weight

Dependent Variable	Preterm		Low birth weight	
<b>Panel A: Work status</b>				
Worked	−0.0020** (0.0008)	−0.0021*** (0.0008)	−0.0027*** (0.0008)	−0.0023*** (0.0008)
Sick	0.0322*** (0.0018)	0.0313*** (0.0018)	0.0314*** (0.0018)	0.0305*** (0.0018)
<b>Panel B: Days worked</b>				
Days worked	−0.0001*** (0.0000)	−0.0001*** (0.0000)	−0.0001*** (0.0000)	−0.0001*** (0.0000)
Sick	0.0312*** (0.0018)	0.0305*** (0.0018)	0.0305*** (0.0018)	0.0298*** (0.0018)
Mother Controls	No	Yes	No	Yes
Mean of Dep. Var.	0.0335	0.0335	0.0327	0.0327
Observations	226,824	226,824	226,824	226,824

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses.

*Notes:* This Table is estimated on the pooled sample of the three RDD regression windows. Additional controls included in all columns are year and month of birth FE, and a gender dummy. Mother's characteristic controls are dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy.

reported in Panels A and B in Table 2.2. The full control model shows that being sick leads at the mean to an increase of 91.0–93.4 percent and 91.1–93.3 percent in preterm birth and low birth weight, respectively. To give some idea of the magnitude of this effect, I can compare this finding with Wüst (2015). She reports an increase in preterm birth of roughly 43 percent at the mean if the mother reports being sick during pregnancy.<sup>22</sup>

Controlling for mother's characteristics only marginally affects the size of the coefficients of interest.<sup>23</sup> However, controlling for a broad set of observables is not sufficient to rule out endogeneity concerns. Therefore, I will exploit the policy reforms in the next sections in a RD setup to infer the impact of prenatal employment on newborn health.

<sup>22</sup>However, Wüst (2015) estimates this effect on the full sample. If I re-run the OLS regression on my full sample as described in Table 2.1 Column (1) including all parities and not only second born, I get an increase of 46–52 percent in a preterm birth if a mother is sick, which is comparable in size to the one reported by Wüst (2015).

<sup>23</sup>Table B.2 reports the entire set of coefficients for mother characteristics.

Table 2.3: OLS Results on Additional Newborn Health Outcomes

Dependent Variable	Birth weight	Gestational length	Apgar 1 min score	Healthy Apgar score
<b>Panel A: Work status</b>				
Worked	11.3360*** (2.1728)	0.0136* (0.0073)	0.0285*** (0.0074)	0.0046 (0.0042)
Sick	-81.5409*** (4.4193)	-0.3414*** (0.0163)	-0.0849*** (0.0084)	-0.0786*** (0.0085)
				0.0005 (0.0007)
				-0.0113*** (0.0015)
				0.0006 (0.0007)
				-0.0104*** (0.0015)
<b>Panel B: Days worked</b>				
Days worked	0.1721*** (0.0152)	0.0007*** (0.0000)	0.0008*** (0.0000)	0.0001*** (0.0000)
Sick	-78.7184*** (4.4173)	-0.3294*** (0.0163)	-0.0833*** (0.0084)	-0.0771*** (0.0084)
				-0.0111*** (0.0015)
				0.0000*** (0.0000)
				-0.0101*** (0.0015)
<b>Mother Controls</b>				
Mean of Dep. Var.	No 3,401.5743	Yes 3,401.5743	No 39.7474	Yes 39.7474
Observations	226,824	226,824	226,824	226,824

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses.

*Notes:* This Table is estimated on the pooled sample of the three RDD regression windows. Additional controls included in all columns are year and month of birth FE, and a gender dummy. Mother's characteristic controls are dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy.

## 2.6.2 First-stage Estimates

*Graphical evidence.* In this section, I test the first stage described in Equation (2.2). In particular, I focus on the extensions of parental leave for the first child and the impact on mothers' employment status during pregnancy with her second child. Figure 2.3 graphically represents the results. Dots refer to monthly averages, to which linearly fitted values and 95 percent confidence intervals are added.

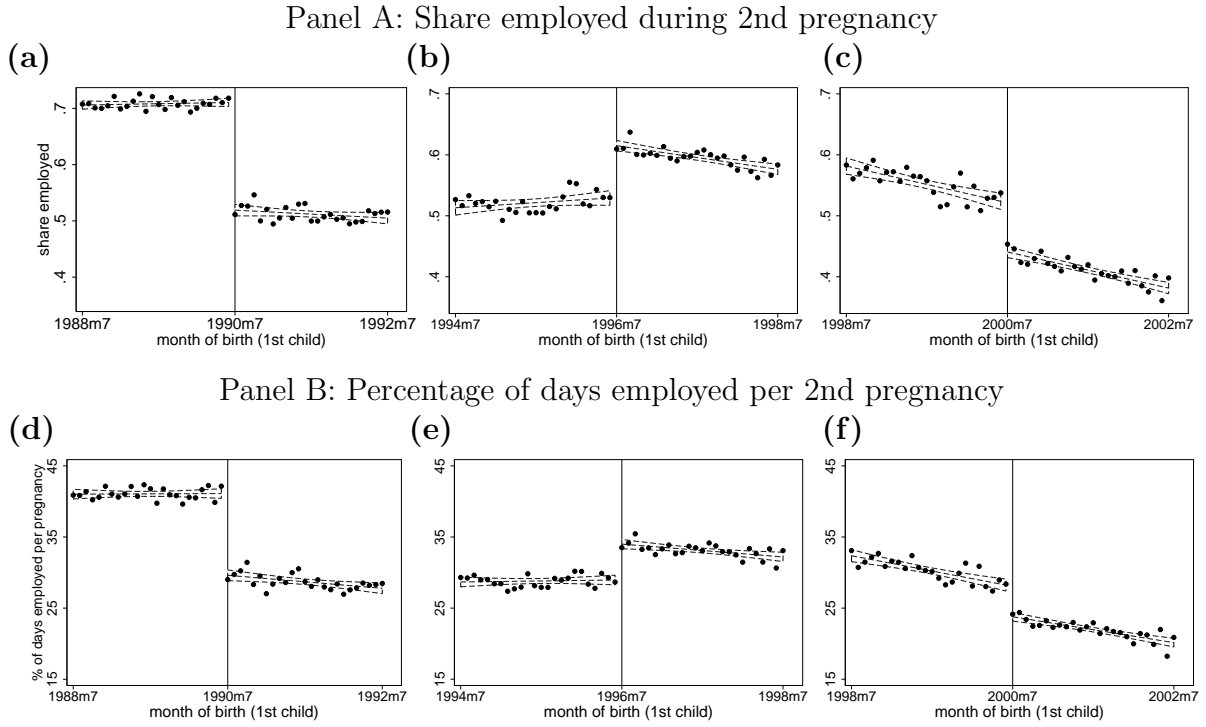
There are statistically significant and economically meaningful discontinuities in the direction as expected around the cutoff dates. The 1990 reform, for example, leads to an approximate decline of 19 percentage points in the share of mothers employed during pregnancy with the second child giving birth to their first child right after July 1990 compared to mothers that gave birth just before the policy reform. The discontinuities for the reforms in 1996 and 2000 highlight the same pattern. When parental leave for the first child declines, as in 1996, the share of mothers employed during pregnancy with their second child increases by approximately 7 percentage points; it declines by 6 percentage points with the extension of parental leave in the year 2000. The intensive margin of days worked corroborates these patterns, where for example in 1990, a 14 percent drop of a potential total of 160 days worked resulted in 22 fewer days worked.<sup>24</sup>

*Estimation results.* Table 2.4 presents regression estimates of Equation (2.2). Each regression is estimated with a chosen bandwidth of 24 months. Columns (1), (3), and (5) show regression results, while columns (2), (4), and (6) add mother's characteristics as controls. All estimates corroborate the graphical evidence found in Figure 2.3 and confirm the statistical significance of the discontinuities in mother's work behavior at the cutoff. In terms of magnitude, the 1990 reform has the strongest impact in the full control

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<sup>24</sup>The 160 days are the product of mothers working 32 weeks during pregnancy times 5 days per week. The 32 weeks are the result of an average pregnancy lasting 40 weeks minus 8 weeks mothers have to spend on maternity leave.

Figure 2.3: RDD Plots Prenatal Employment



*Notes:* This Figure reports the fraction of mothers working during pregnancy with their second child and the average percentage of days employed per pregnancy by month of birth of the first child. All subfigures are based on the full sample of matched and eligible mothers that gave birth to their first child not more than 24 months apart from a policy reform.

model on maternal employment during pregnancy with her second child, decreasing the percentage of mothers employed and the days employed by 19.1 percentage points and 23.0 days, respectively. The corresponding numbers for the 1996 reform show an increase of 7.2 percentage points and 9.5 days in the employment share and days worked. The 2000 reform leads to a decline of 6.4 percentage points in the percentage of mothers working and a decline of 6.6 days in the days worked. Altogether, the documented effects are significant, precisely estimated, and economically important.

The point estimates of the effect of having an older sibling born post-policy reform on the work status and the days worked do not vary with the inclusion of predetermined mother's controls. This robustness to the inclusion of additional controls confirms the validity of the RDD in this setting. Furthermore, the regression results are robust to other choices of bandwidths and functional forms (see Figure B.3; Table B.3).

Table 2.4: RDD Effects on Maternal Employment During Pregnancy

	1990	1996	2000			
Panel A: Dependent variable employment status						
1{Post policy reform}	-0.191*** (0.006)	-0.191*** (0.006)	0.069*** (0.007)	0.072*** (0.007)	-0.064*** (0.008)	-0.064*** (0.008)
Comparison Mean	0.714	0.714	0.587	0.587	0.600	0.600
Panel B: Dependent variable days worked						
1{Post policy reform}	-23.105*** (0.924)	-23.011*** (0.891)	9.147*** (0.980)	9.493*** (0.954)	-6.712*** (1.083)	-6.636*** (1.059)
Comparison Mean	82.720	82.720	65.607	65.607	67.369	67.369
Additional Mother Controls	No	Yes	No	Yes	No	Yes
Observations	87,566	87,566	77,279	77,279	63,481	63,481

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses.

*Notes:* This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth just after the policy reform versus just before. Panel A estimates the effects on employment status and Panel B on the number of days worked during pregnancy with the second child.



### 2.6.3 Reduced-form Estimates

*Graphical evidence.* This section reports the reduced-form estimates of Equation (2.3). In particular, I analyze the impact of the extension of parental leave for the first child on several health outcomes for the newborn child, such as being born preterm or with low birth weight. These reduced-form estimates can be interpreted as the net effect of the policy reforms on newborn health. As the first-stage estimates were generally large and statistically significant, we would expect to see sizeable effects on newborn health if there is a relationship between the latter and a mother's work status.

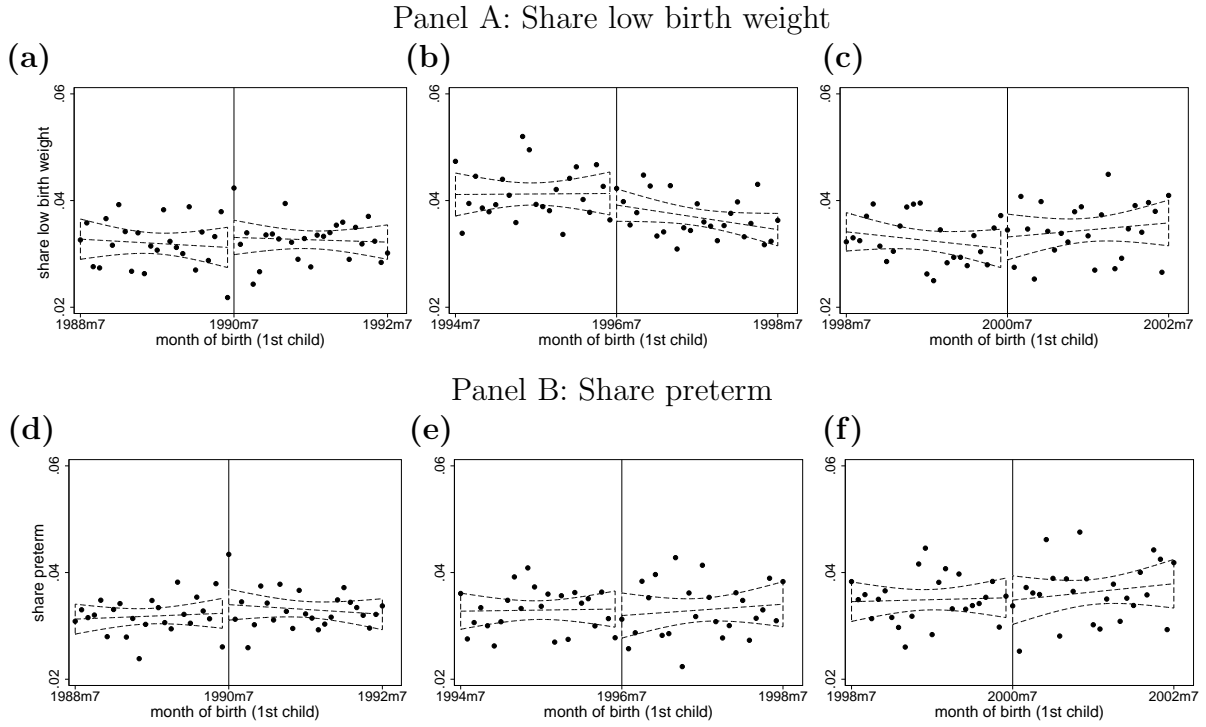
Figure 2.4 shows the effects of the policy changes on newborn health. Dots represent monthly averages and solid lines correspond to values from a linear fit. There is no significant discontinuity visible. This holds true for any of the outcome variables and also for all three policy reforms. Linking these results with the discontinuous jump found for the first-stage results of prenatal employment leads to the conclusion that my estimates are consistent with a null hypothesis of no effect of maternal employment during pregnancy on newborn health.

*Estimation results.* The graphical results are complemented with regression outputs estimating Equation (2.3) in Table 2.5. All columns include a gender dummy, year of birth and month of birth fixed effects. Columns (1), (3), and (5) show regression results with child-level controls only, while columns (2), (4), and (6) add mother's characteristics controls. All estimates support the graphical evidence found in Figure 2.4. For all three reforms and all newborn health outcomes, I document statistically insignificant and generally small effects. For example in 1990, the estimates allow me to rule out an increase of more than 0.6 percentage points and a decline of more than 0.2 percentage points in the probability of being born with low birth weight.<sup>25</sup> Furthermore, the point estimates are

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<sup>25</sup>This is the result of a 95 percent confidence interval, leading to a positive effect of a maximum of +0.006 ( $=0.002+1.96*0.002$ ) and a minimum of -0.002 ( $=0.002-1.96*0.002$ ).

Figure 2.4: RDD Plots Newborn Health



*Notes:* This Figure reports the average share of second children being born with low birth weight and preterm by month of birth of the first child (their older sibling). Note that I introduce a shifter for subfigure (b). Birth weight has been reported in hectograms up to December 1998. From January 1999 birth weight is measured in decagrams. This switch goes hand in hand with a discontinuous increase in birth weight and decrease in the probability of low birth weight most likely due to a rounding down previous to 1999. I correct for it in these graphs by multiplying birth weight observed after the shift with the change in yearly average values from 1998 to 1999. All subfigures are based on the full sample of matched and eligible mothers that gave birth to their first child not more than 24 months apart from a policy reform.

not affected by the inclusion of mother's characteristics controls. Overall, these results indicate that the net impact of the extension of parental leave duration for the first child and the accompanying significant reduction in prenatal employment during pregnancy with the second child on newborn health is negligible.

## 2.6.4 Robustness Tests

In this section, I test the robustness of my results. For most of the reported results, I will only focus on the 1990 reform because all conclusions hold true for the other reforms.

*Alternative outcome measures.* Figure 2.5 reports results on additional outcome mea-

Table 2.5: RDD Effects on Newborn Health of Second Born

	1990		1996		2000	
<b>Panel A: Dependent variable low birth weight</b>						
1{Post policy reform}	0.002 (0.002)	0.002 (0.002)	-0.004 (0.003)	-0.004 (0.003)	0.001 (0.003)	0.001 (0.003)
Comparison Mean	0.031	0.031	0.034	0.034	0.031	0.031
<b>Panel B: Dependent variable preterm</b>						
1{Post policy reform}	0.001 (0.003)	0.001 (0.002)	-0.002 (0.003)	-0.002 (0.003)	-0.001 (0.003)	-0.001 (0.003)
Comparison Mean	0.034	0.032	0.033	0.033	0.035	0.035
<b>Additional Mother Controls</b>						
Observations	No 87,566	Yes 87,566	No 77,279	Yes 77,279	No 63,481	Yes 63,481

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses.

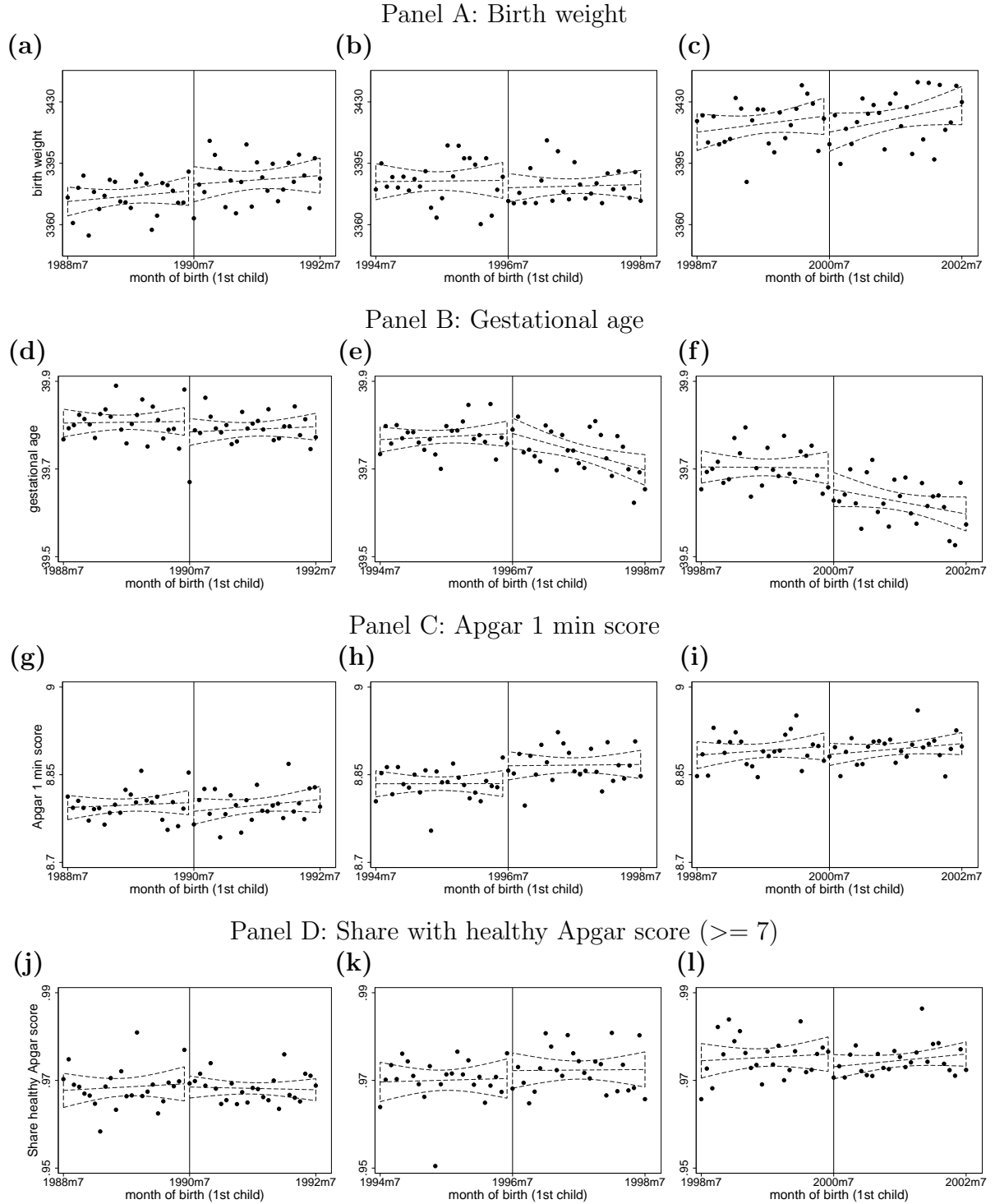
*Notes:* This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. All columns include a gender dummy, year of birth and month of birth fixed effects. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth just after the policy reform versus just before. Panel A estimates the effects on low birth weight and Panel B on being born preterm.

asures such as birth weight, gestational length, 1 min Apgar score, and a dummy for a healthy Apgar score. As with the two main outcome measures discussed so far, there are no visible discontinuities around the thresholds. This is convincing that there is no effect of the reform on any newborn health measure. While preterm and low birth weight dummies might capture effects on very vulnerable newborns, the continuous variables of birth weight and gestational age and Apgar scores measure potentially another dimension of newborn health.

*Different birth weight and gestational age thresholds.* In Figure B.4, I report the reduced-form effect for separate regressions with different choices of birth weight and gestational age cutoffs. While low birth weight, defined as less than 2,500 grams, and being born preterm, defined as less than 37 weeks of gestational age, are relatively arbitrary cutoffs, I show that the results are robust to different choices for these outcome variables. No matter what birth weight and gestational age threshold is chosen, the effect of the policy reforms on these birth outcomes is statistically insignificant.

*Donut estimations.* Panel A of Table 2.6 shows results of Donut estimations, which create a hole in the middle of the sample. Here I exclude data points from second borns with older siblings born within a week on both sides of the cutoff. As all policy reforms were announced only shortly before the actual implementation, selection into motherhood and therefore a perfect planning of birth into a specific policy regime can be ruled out. However, the choice of a Caesarean section and therefore timing the birth might still be possible up to a short time window of around 1 week. Lalive and Zweimüller (2009) investigate the issue of this narrow-window timing and argue that although there is a steady increase in births on a day-to-day basis from June to July, there is no discontinuity in the reported births on July 1. Also in my analysis, results are robust to this adjustment and all conclusions hold true.

Figure 2.5: RDD Plots Newborn Health Additional Outcome Measures



*Notes:* This Figure reports the average birth weight, gestational age, 1 min Apgar score and the share with a healthy Apgar score ( $\geq 7$ ) of second children by month of birth of the first child (their older sibling). Note that I introduce a shifter for the subfigure (b) as outlined in the *Notes* of Figure 2.4. All subfigures are based on the full sample of matched and eligible mothers that gave birth to their first child not more than 24 months apart from a policy reform.

Table 2.6: Robustness: 1990 Reform

Dependent variable	Worked	Days worked	Low birth weight	Preterm
Panel A: Excluding 1 week around cutoff				
1{Post policy reform}	-0.191*** (0.007)	-22.961*** (0.942)	-22.848*** (0.908)	-0.000 (0.003)
Observations	86,702	86,702	86,702	86,702
Panel B: Adding third born				
1{Post policy reform}	-0.167*** (0.006)	-20.050*** (0.811)	-19.958*** (0.785)	-0.001 (0.002)
Observations	113,294	113,294	113,294	113,294
Panel C: Bounds on Treatment Effects				
Treatment Bounds	[ -0.194, -0.127]		[ -0.003, 0.008]	
Panel D: Placebo cutoff July 1988				
1{Post policy reform}	0.000 (0.006)	-0.243 (0.947)	-0.123 (0.920)	0.001 (0.002)
Observations	80,754	80,754	80,754	80,754
Comparison Mean	0.714	82.720	0.031	0.032
Comparison Mean incl. 3 born	0.684	78.625	0.034	0.034
Comparison Mean 1988	0.708	82.331	0.032	0.029
Additional Mother Controls	No	No	Yes	No
			Yes	Yes

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses.

*Notes:* This Table is estimated on the matched and eligible sample of second born (and 3d born in Panel B) with an older sibling born within a bandwidth of 24 months around the policy reforms. Standard errors in the treatment bound estimation reported in Panel C are calculated via bootstrapping. Columns (5-8) include a gender dummy, year of birth and month of birth fixed effects. In Panel B Columns (5-8) additionally include birth order dummies. Additional Mother Controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first (or previous in Panel B) birth just after the policy reform versus just before. In Panel A births that happen within around 1 week of a policy change are excluded. Panel B additionally also includes births of third born, Panel C implements bounds on the treatment effect under selection into second motherhood, and Panel D shows Placebo estimates for an imaginary reform in July 1988.

*Adding third born.* In Panel B of Table 2.6, I add the sample of third born with an older sibling born 24 months around the cutoff of a policy reform. Although the sample size and therefore the precision increase, Panel B detects no major changes to the previous conclusions. Mothers are significantly less likely to work during pregnancy with their higher-order child if facing longer parental leave with their older child. Effects on newborn health measures for their consecutive child are still not significant. This highlights that the results are not specific only to second borns.

*Sample selection.* Panel C of Table 2.6 addresses the previously raised concern of selection into second motherhood by reporting bounds on treatment effects in the presence of possible selection. Applying the monotonicity assumption for the 1990 reform yields the bounds reported in Panel C in Table 2.6, where I draw the same conclusions as before. Maternal employment during pregnancy with the second child decreases significantly with no significant effects on newborn health for the second child.

*Placebo reform 1988.* In Panel D of Table 2.6, I show estimation results of a placebo regression assuming a policy reform in July 1988. As expected, this placebo treatment in a non reform year shows no significant effects for any of the outcome variables. Interestingly, as this is known to be a true zero effect, I can compare the standard errors of this specification with the results reported for the three reforms in Table 2.5. The size of the standard errors of the two outcome measures is very similar for the placebo and the reform regressions.

## 2.7 Discussion

*Heterogeneity.* I stratify my sample according to several different maternal characteristics.<sup>26</sup> I start by analyzing broad subsamples in which I classify mothers into blue versus

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<sup>26</sup>Information about mother's characteristics refer to the first pregnancy to mitigate possible endogeneity concerns.

white collar, low versus high socioeconomic status, and married versus unmarried. Several considerations give rise to these broad classifications. Stratification by occupational collar, for example, allows me to differentiate between manual labor versus office work due to its correlation with job task. A priori, it is not clear whether one would expect more beneficial effects for one or the other, as different types of job tasks could have varying effects over the pregnancy cycle. Job protection might favor high-SES mothers where her skills are more attached to her current job, while paid leave might be more beneficial for low-SES mothers. Finally, one might expect married mothers to react stronger in decreasing prenatal employment due to longer leave, as they are in a stable family environment and thus are more likely to plan subsequent fertility.

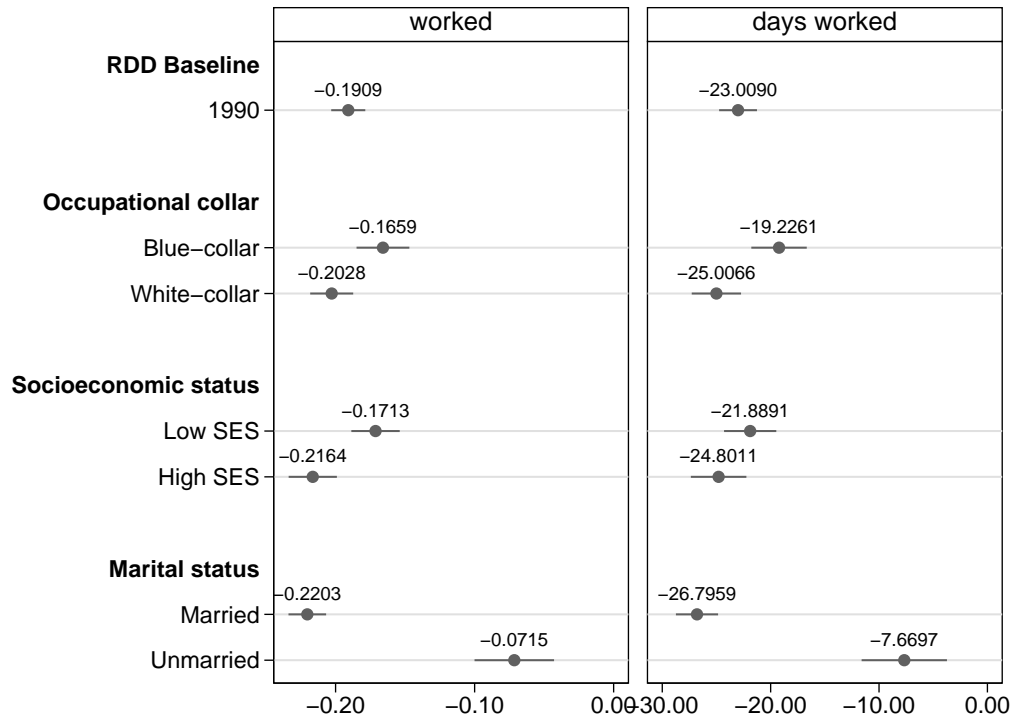
Results for these subgroups are reported in Figure 2.6 and are in line with expectations. There are differences across subgroups with respect to prenatal maternal employment; however, except for married versus unmarried mothers, these are not large and the effect for each subgroup per se is sizeable and significantly different from zero. Unlike prenatal employment, the general finding for newborn health measures across all reported subgroups is not statistically different from zero. Standard errors become larger for smaller subgroups (such as unmarried mothers for example), but all estimates are relatively small in absolute terms. The only notable difference occurs across socioeconomic status where there is a sign switch for the effect of reduced prenatal employment on the likelihood of a preterm birth. However, as both effects are still indistinguishable from zero, in a next step, I refine mothers by income quintiles.

Results for mothers by income quintiles are reported in Figure 2.7. Interestingly, the results by socioeconomic status, which were insignificant but of opposing sign, become statistically significant despite smaller sample groups. All mothers are less likely to work during pregnancy after the 1990 reform and show sizeable reductions in prenatal employ-

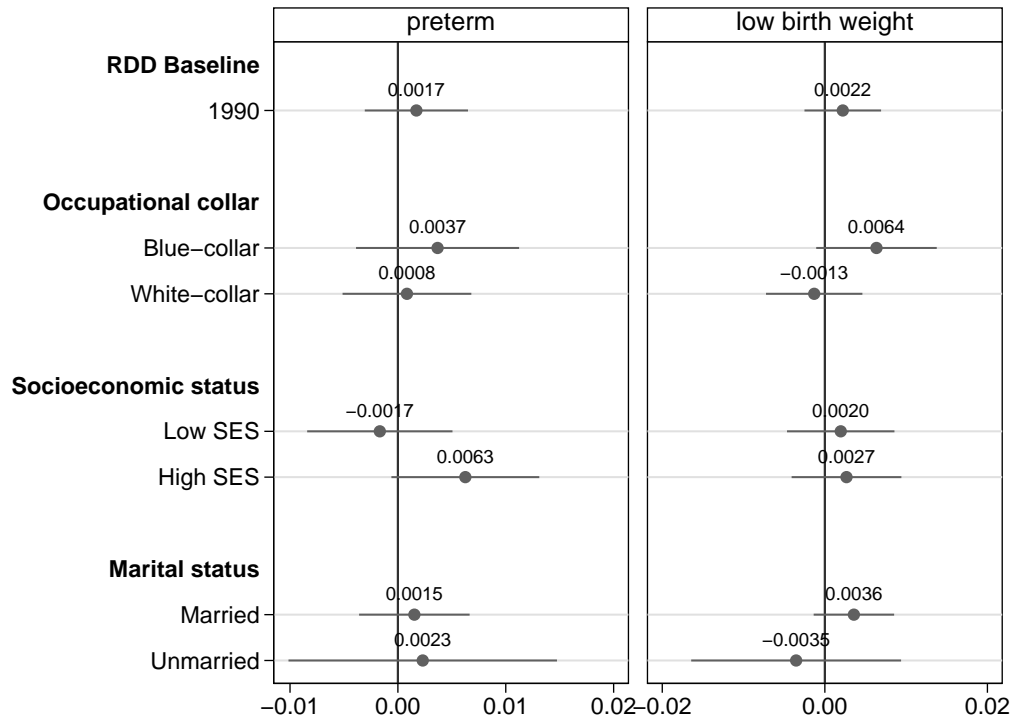


Figure 2.6: RDD Estimates General Heterogeneity

Panel A: Work estimates



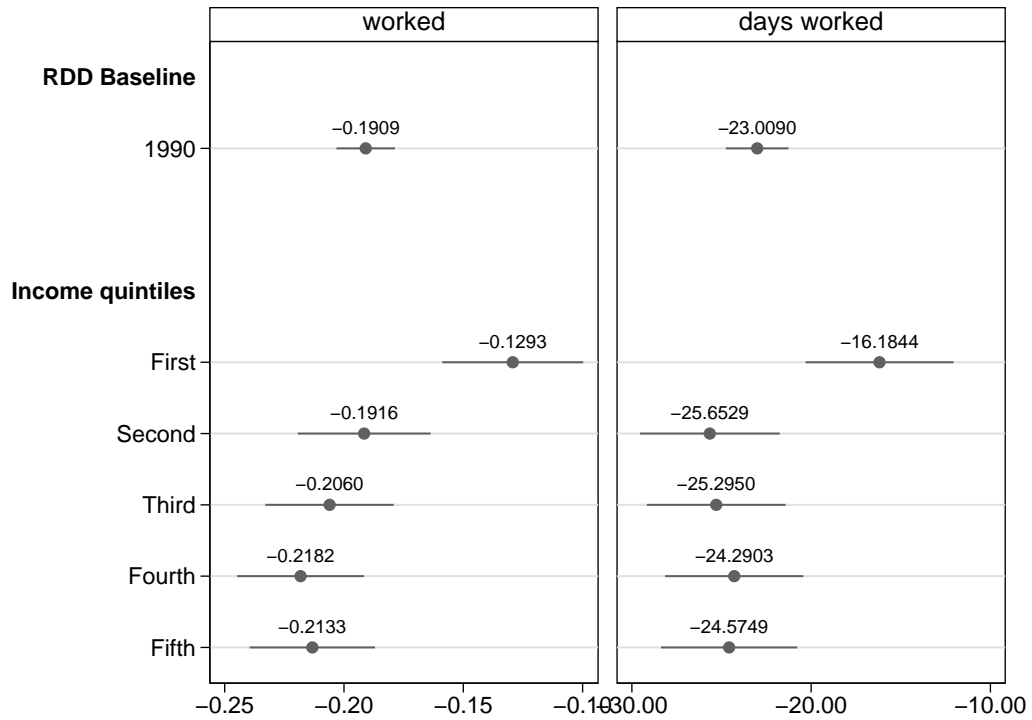
Panel B: Newborn health estimates



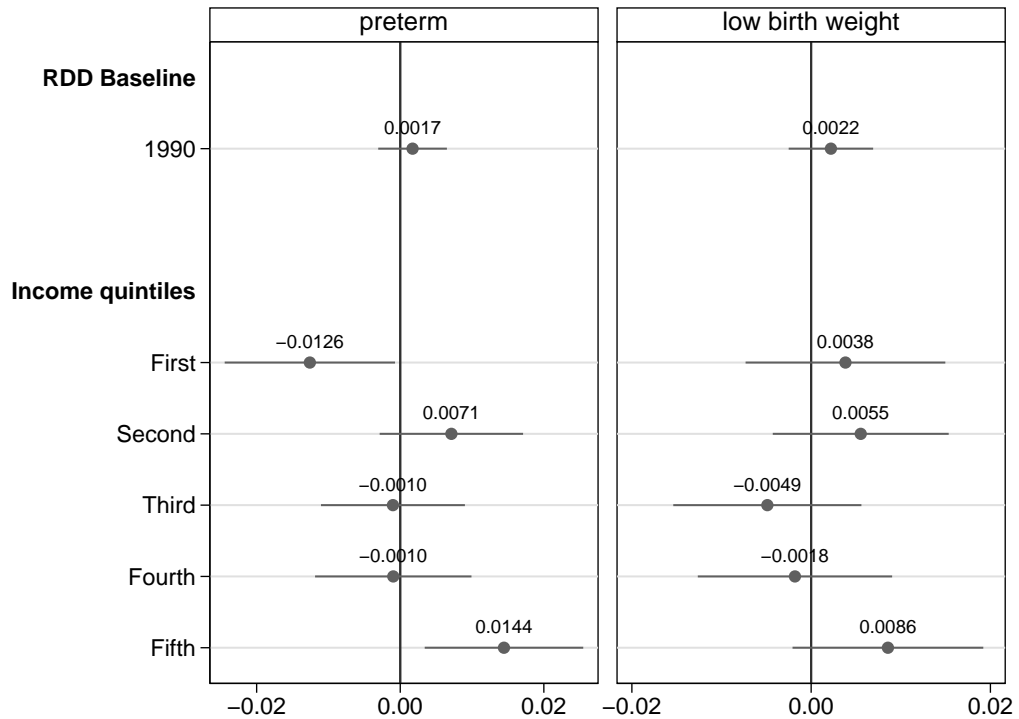
*Notes:* This Figure reports in Panel A (B) parameter estimates for different specifications for the coefficient  $\gamma_1$  ( $\delta_1$ ) of having an older sibling born post policy reform on work status and days worked (low birth weight and preterm). Regression coefficients with a 95 percent confidence interval are displayed. The sample consists of all matched and eligible mothers that gave birth to their first child not more than 24 months apart from the respective policy reform.

Figure 2.7: RDD Estimates Heterogeneity by Income Quintiles

Panel A: Work estimates



Panel B: Newborn health estimates



*Notes:* This Figure reports in Panel A (B) parameter estimates for different specifications for the coefficient  $\gamma_1$  ( $\delta_1$ ) of having an older sibling born post policy reform on work status and days worked (low birth weight and preterm). Regression coefficients with a 95 percent confidence interval are displayed. The sample consists of all matched and eligible mothers that gave birth to their first child not more than 24 months apart from the respective policy reform.

ment from 12.9–21.3 percentage points. The reduction in prenatal employment leads to better outcomes for their newborns (i.e. a reduction in preterm births by 1.3 percentage points) for mothers in the lowest income quintile and to worse outcomes for their newborns (i.e. an increase in preterm births by 1.4 percentage points) for mothers in the highest income quintile. These results can be explained by income effects during pregnancy. The parental leave policy is designed as flat benefit, which describes an income improvement for some mothers and an income decline for others. The increase in income for low-income mothers and the reduction in preterm births is in line with previous literature (Hoynes et al., 2015; Almond et al., 2011; Barber and Gertler, 2008; Amarante et al., 2016). More interestingly, and also substantially less studied in the literature, is the harmful effect on newborn health of decreasing income for high-income mothers.

While the stratification by income quintile hints toward an income effect, the effect cannot be explained by differential occupational exposures.<sup>27</sup> I also conduct subgroup analysis by industry and find no significant effects on newborn health measures. This is interesting as one might expect different types of occupations to expose mothers to different types of risky behavior during pregnancy, as for example, smoking in the hospitality industry or diseases in the human health and social work industry.

I also test for other heterogeneous subgroups such as gender of the child, above median age of the mother, country of origin of the mother, and health outcomes of the preceding child (i.e. born with low birth weight). I find a zero effect on all newborn health measures for all of these subgroups.

*Age Difference.* Although the mean difference in months from the first to second born is not affected by any of the policy reforms, certain specific age windows could be affected by the design of the policy reforms. From a medical point of view, siblings with an age

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<sup>27</sup>Results are not reported here but are available upon request.

difference of less than 18 months are particularly interesting to look at more closely, as this very short spacing can lead to worse outcomes of the second born (Conde-Agudelo et al., 2006). Therefore, for a better understanding of the age difference between first- and second-born children, I decompose the effect into respective age categories implied by the three policy reforms. Table B.4 reports the results. An extension of parental leave as in 1990 and 2000 reduces the likelihood of very small age differences, while a reduction in parental leave increases the likelihood of small age differences. The effect is significant for the 1990 and 1996 reforms. Thus, if short spacing would harm the health of the second-born child, this would bias the effect of prenatal maternal employment on newborn health upwards in 1990 and downwards in 1996. However, in terms of magnitude, both effects are small.

## 2.8 Conclusion

This paper analyzes the effect of maternal employment during pregnancy on newborn health. I use data on all Austrian births and work histories of the respective mothers. To overcome the endogeneity of employment decisions, I exploit three policy reforms regarding the duration of parental leave. I find no evidence that prenatal employment affects any of the outcomes, measured via birth weight, gestational age, and Apgar scores. This stands in contrast to the positive correlation of prenatal employment with newborn health as reported in the OLS regressions.

For the 1990 reform, which extended parental leave from one to two years, I document a significant 19.1 percentage point decline in the share of mothers employed during pregnancy with their second child and insignificant effects for all newborn health measures. Even though prenatal employment varies substantially by subgroups, I find no evidence of an effect on health outcomes at birth even in these subpopulations. The subgroup analysis

by income quintiles is the only one to reveal significant effects on newborn health, which can be explained by income effects (i.e. higher income for poorer mothers leads to better newborn health).

This study shows that the net effect of changes in parental leave policies for the second child is insignificant. This result is robust to the inclusion of covariates and several different specifications. The zero effect of prenatal employment adds to the scarce and conflicting literature on maternal employment during pregnancy on newborn health. With increasing employment rates for pregnant women, it is crucial to understand the mechanisms of prenatal employment on newborn health.

The literature on the effects of pregnancy conditions on long-term outcomes identifies the second trimester of a pregnancy as the trimester with the strongest neural brain development (Schwandt, 2018; Black et al., 2018). This could mean that the effect of exposure to maternal labor market participation during pregnancy could only be detectable in the long run and may not be visible on the day of birth. As such, linking prenatal employment with long-term outcomes such as educational attainment and labor market participation of the child itself could be explored in future studies.

The findings in this paper should interest policymakers and pregnant women. Unlike post natal child rearing, the duty of pregnancy cannot be shared among partners. However, this should not worry mothers or employers, as there is no evidence that the working status during pregnancy leads to bad health outcomes for the unborn baby. However, the results should also be interpreted within the Austrian context, where women are relatively well protected against hazardous situations at their workplace while pregnant. Furthermore, the results of this paper will help women to optimally allocate the time allowed by parental leave policies. Several countries design their parental leave policies in such a way that women can choose how to divide a certain amount of weeks pre- and post-birth.

These results show that women can safely take a majority of their parental leave after giving birth.

# Chapter 3

## Baby Bonus in Switzerland: Effects on Fertility, Newborn Health, and Birth Scheduling

*Joint with Patrick Keller*

### 3.1 Introduction

Having children is expensive. Therefore, several family policies are put in place to support families financially. Birth allowances, a one-time payment at the event of giving birth, are designed specifically for the vulnerable transition from being a couple without a child to becoming new parents. Introducing this financial support might affect two margins of parental behavior: the short- and the long-run.

In the long-run, birth payments can affect fertility by incentivizing couples to become parents. This is an especially important topic for countries with an aging population and fertility rates below the replacement level of 2.1 children per woman. As such, it may be in the society's interest to boost fertility by providing financial incentives.

In addition to that, in the short-run the announcement of a new baby bonus policy can affect birth scheduling and consequently newborn health. Financial incentives, for example, may motivate parents to shift a birth both forward or backward. This can have severe long-run consequences for the unborn child, as advancing or postponing a birth affects newborn health. Newborn health, in turn, has been linked to later-life outcomes as summarized by Almond and Currie (2011) and more recently by Almond et al. (2018).

In this paper we study the effect of introducing, increasing, or abolishing birth payments on fertility, newborn health, and birth scheduling. For the empirical analysis we draw on several administrative data sets from 1969 to 2017. We build several outcome variables based on the Swiss birth register, the universe of all stillbirths, and the statistics on infant deaths.<sup>1</sup> Combining these outcome variables with cantonal information on birth allowances allows us to study the causal impact of birth payments in the unique quasi-experimental setting of Switzerland. Based on this, we implement a difference-in-differences estimation. We conduct several sensitivity analyses by adjusting the baseline estimation including only ever treated cantons, excluding early movers, and implementing everything at the municipal level.

Family allowances in Switzerland are federally organized. However, the authorities leave a certain degree of freedom for the exact design of the baby bonus on the cantonal level. As such, cantons are free to choose whether they want to implement birth payments or not. Additionally, cantons can choose the amount they want to pay at any point in time. A birth payment is a one-time payment in the event of birth, transferred to the mother residing in a canton that chose to pay a baby bonus. In the studied time period, 11 out of 26 cantons have a baby bonus put in place. Three of these cantons introduced

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<sup>1</sup>A stillbirth is defined as a death that occurs when the baby is still in a mother's womb. It is differentiated from a miscarriage by meeting two criteria: First, the gestational length has to be at least 22 weeks and second, the unborn must weigh at least 500 grams. In Switzerland, every stillbirth (in contrast to a miscarriage) has to be reported. Every death that occurs after birth and under age one is counted as an infant death.



a birth allowance already before the onset of the available data in 1969. Another two cantons abolished the birth allowance after several years and all 11 cantons frequently adjust the payment. Some of these adjustments are as high as doubling the previously paid amount.

Our results of introducing a baby bonus in Switzerland show a positive effect on the crude birth rate, which is significant in our specification where we include only ever treated cantons. This positive effect is relatively smaller for first-births compared to higher-order births, suggesting that the intensive margin of having additional children is more strongly affected. Furthermore, we find a significant and sizable reduction in stillbirths. The birth allowance reduces the stillbirth rate by up to 23 percent. A possible channel for this effect could be a reduction in stress, due to the baby bonus — which might occur especially among low-income parents. We confirm this hypothesis in our heterogeneity analysis, where we report a stronger impact on the reduction of the stillbirth rate for older and foreign mothers. In line with this positive health impact, we also report a significant increase in the birth weight of around 0.6 percent evaluated at the mean. All of these results are robust to several sensitivity checks.

Despite the impact on fertility and newborn health on the aggregate level, we do not find birth scheduling around the policy changes. We argue that this is the result of several features in the Swiss setting. First, changes are in absolute terms smaller than in other countries with birth allowances. Second, most of these changes are not covered enough in local media and third, the number of observations, i.e. the total birth count on the daily cantonal level, may just be too little in order to document significant changes even when combining all reforms across cantons.

This paper contributes to the literature on the impact of cash transfers on fertility behavior and to the literature on policy announcement effects on birth scheduling and

newborn health. This literature emphasizes that parents do react to financial incentives in adjusting the overall fertility behavior and the scheduling of births.

The most closely related birth scheduling studies by Gans and Leigh (2009); Tamm (2013); Neugart and Ohlsson (2013); Brunner and Kuhn (2014); Borra et al. (2019) analyze birth allowances in other countries. Gans and Leigh (2009) study the implementation (and extension) of a large and shortly announced baby bonus in Australia in 2004 (2006). They find sizable birth shifting with heavier babies of which a quarter were shifted more than one week. Tamm (2013) and Neugart and Ohlsson (2013) study a shift in the German parental leave system in 2007. They find that the reform encouraged parents to postpone births due to higher benefits paid within the first two years after birth. Brunner and Kuhn (2014) analyze the abolition of the baby bonus in Austria in 1997. Due to the announcement of the policy change 10 months prior to implementation, they find a large fertility impact in the month before the policy shift. As no health impacts on newborns can be detected, they argue that this is rather a fertility effect than a birth scheduling effect. Borra et al. (2019) investigate the abolition of the Spanish baby bonus in 2010. The policy was announced seven months prior to its implementation and parents react by shifting births forward with severe negative health consequences for their newborns.

There exist also several papers that investigate tax incentives and birth scheduling in the United States (Dickert-Conlin and Chandra, 1999; Schulkind and Shapiro, 2014; LaLumia et al., 2015). Using data on the universe of births in the United States for different time periods, all of these papers show that parents are incentivized by the tax scheme to schedule births in late December instead of early January.

More generally, there is also a large and growing strand of the literature analyzing the impact of cash transfers on fertility behavior (Kearney, 2004; Milligan, 2005; Cohen et al., 2013; Laroque and Salanié, 2014). Several of these studies find a sizable impact on fertility

when parents face financial support. Most closely related to our study is Milligan's (2005) analysis of a Canadian policy reform, which led to transfers up to CAD 8,000 (roughly CHF 6,000) for the third child. He finds a strong effect on fertility. While the absolute amount paid is significantly higher in the Canadian study, the incentive scheme differs in the eligibility criteria. The Canadian policy mostly animates parents with an existing two children to get a third child. Thus, this affects fertility at the intensive margin. The Swiss transfers are substantially smaller in absolute value for all cantons, but are already being paid for the first child. Therefore, the Swiss case allows to study fertility effects at the extensive margin. We expect the extensive margin more difficult to affect, because the marginal cost of an additional child are presumably decreasing.

Our paper contributes to these two strands of the literature in various ways. First, we have a plausible control group. Due to the quasi-experimental setting in the Swiss context, we are the first to introduce a control group: Cantons, which never introduced any birth allowances. Previous studies always analyzed national policy changes instead of cantonal policy changes. Our setting does not only improve the causal interpretation of the reported estimates, it also enables us to study both short-run (birth scheduling) and long-run (fertility responses) behavior as we can rely on a difference-in-differences structure.

Second, we have a long time horizon. The panel structure of the data and the long history of Swiss family allowances, allows us to study a differential impact of baby bonuses over time. This is interesting due to a variety of reasons. Not only the institutional setting, and thus the role of women in society has changed a lot over the last century, but also the medical technology concerning the pregnancy and the birth itself have vastly improved.

Third, we can analyze introductions, increases, and abolitions of the baby bonus within one country. This is of special interest in the birth scheduling analysis. The inherent

setting allows us to study asymmetries as the parental choice of delaying or scheduling a birth early is a different one. While delaying a birth is unlikely to put the newborn's health at risk, it might not be feasible due to the natural end of a pregnancy. Inducing a birth early, however, is feasible with the available medical interventions — at the possible costs of compromising a baby's health.

Finally, we can generalize the literature of cash transfers on fertility, as we can study not only the intensive but also the extensive margin of couples becoming new parents. Furthermore, we can study various heterogeneous effects, as the birth payment is paid to every mother without any eligibility criteria. This stands in contrast to most of the previous literature which analyzed fertility effects of cash transfer programs which were targeted toward low-socioeconomic status (SES) families.

The remainder of the paper is organized as follows. Section 3.2 describes the institutional background on birth allowances in Switzerland. Section 3.3 describes the data used for the empirical analysis and Section 3.4 introduces the empirical strategy. We present various results and sensitivity analyses on fertility and newborn health in Section 3.5 and on birth-scheduling in Section 3.6. We discuss these results in Section 3.7. Finally, Section 3.8 concludes.

## 3.2 The Swiss Baby Bonus

Switzerland has a decentralized federalist political system. There are three interdependent governmental levels: the federal, cantonal, and municipal level. Family allowances are regulated on the federal level. However, each canton has the authority to adjust the local payments individually. There are three different types of family allowances: (1) child allowances, which by federal law since 2009 have to be at least CHF 200,<sup>2</sup> (2) education

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<sup>2</sup>The evolution of child allowances over time are depicted in Figure C.1 in the Appendix.

allowances, which by federal law have to be at least CHF 250, and (3) the birth allowance with no federal minimum payout. Thus, cantons are generally free to implement a baby bonus and — if implemented — to define the amount paid. They may also increase the baby bonus or abolish it all over at any point in time. This gives rise to large variation across cantons.

An important difference between these benefits is that child and education allowances are monthly money transfers while the baby bonus is a one-time payment. While the different forms of allowances may change at the same point in time, eligibility to collect one type of allowance varies. All mothers residing in a specific canton and giving birth after the implementation date of the baby bonus, are eligible for the baby bonus. Thus, there is a sharp cutoff from one day to the next. For child and education allowances, every child eligible in a month can benefit from higher payments after a policy change.<sup>3</sup> This is to clarify that in practice the baby bonus and the child allowance paid in the first month after birth never offset each other.

In this paper, we will only focus on birth payments on the grounds that the unique setup of this benefit allows us to analyze newborn health, birth scheduling, and fertility effects. The birth payment is a unique payment to a woman who had a living birth. Payments are also granted in case of a stillbirth after at least 23 weeks of gestation. The birth payment is per newborn. For a multiple birth a mother can collect the baby bonus for each child. In that case, the individual payment per child may be higher than for a single child. The only condition, which the monetary transfer depends on, is the mother's canton of residence.

The baby bonus may affect two outcome margins. Mothers may want to shift their birth in order to become or stay eligible for the birth payment. This is the short-run

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<sup>3</sup>Eligibility for child and education allowances depends on the age of the child, the educational track of the child, and the employment status of the parent.

margin, which may affect newborn health in case of birth scheduling. This effect is not diluted by a change in the fertility behavior as all policy changes were announced no more than seven months before the implementation.<sup>4</sup> Therefore, at the time of the announcement mothers were already pregnant. In the long-run, however, mothers might also adjust their fertility behavior, which is the second margin of adjustment.

Birth scheduling is the result of financial incentives of introductions, increases, and abolitions of birth payments. On the one hand, births may be delayed beyond the date of implementation when a baby bonus is introduced or substantially increased. On the other hand, births may be brought forward when a baby bonus is abolished. It is more difficult to delay a birth than to schedule early, due to the natural end of every pregnancy. There are several ways to delay labor (Coomarasamy et al., 2003; Shapiro et al., 2013; Lima et al., 2018): One is to avoid stress or to take medication to delay labour by up to 48 hours. Another one is to postpone an already planned Caesarean section. Through a delay, a newborn is expected to have a higher weight and length at birth, since the unborn had more time to grow in the mother's womb.

In the case of an abolition, mothers may want to accelerate the pregnancy. Mothers can schedule a birth early via a Caesarean section or to induce labour medically. These choices will lead to an earlier birth and a lighter and shorter newborn. As a consequence, mothers have to weight financial gain against the health of their newborn.

In the long-run, higher birth allowances can increase fertility. The predominant reason for introducing birth payments in other countries are to boost fertility. Thus, it is not unlikely to affect fertility in Switzerland as well. This can be tested, as the cantons which never introduced any payment can serve as control. Furthermore, the payment may also improve newborn health. This channel is expected to be especially strong for parents with low-socioeconomic status. For example, financially distressed parents may benefit

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<sup>4</sup>See for further information on announcement and implementation dates Table C.1 in the Appendix.

from this extra payment and negative pregnancy outcomes, such as a stillbirth, might be prevented and positive birth outcomes, such as higher birth weight may be promoted.

### 3.3 Data

We base the empirical analysis on several data sources. The data on all outcome measures such as newborn health, birth scheduling, and fertility is coming from the Swiss Vital Statistics and Annual Population Statistics provided by the Federal Statistical Office (FSO). The Swiss Vital Statistics comprise the three data sets on the universe of births, stillbirths, and deaths. Information on the amount and the date of implementation of all birth allowances per canton is recorded by the Federal Social Insurance Office (FSIO).

#### 3.3.1 Data Sources

*Swiss Birth Register.* The Swiss birth register covers all births from 1969 to 2017. It contains information on the exact date of birth, sex of the child,<sup>5</sup> and beginning in 1979, birth outcomes, such as weight at birth and length at birth. Furthermore, it provides information on birth order and the birth interval in months to a preceding birth if applicable. To calculate the crude birth rate per 1,000 people and the total fertility rate per woman, the data is merged on a canton-year level with the Annual Population Statistics.<sup>6</sup> The Annual Population Statistics is generally available from 1971, with detailed information on age-specific population starting in 1981. Thus, the crude birth rate can be reported from 1971 onward, while the total fertility rate is only available after 1981.

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<sup>5</sup>We study sex ratios as one of our outcome variables. There exist several arguments for how socioeconomic conditions can affect the sex ratio as summarized by Scalone and Rettaroli (2015). Improving socioeconomic conditions can on the basis of the biological argument favor boys as the male fetus is known to be frailer. As an evolutionary explanation better socioeconomic conditions can also lead to more boys due to the reproductive success argument.

<sup>6</sup>We follow conventional definition to measure these two rates. The crude birth rate is the total number of births divided by the total population multiplied by 1,000. The total fertility rate is the result of dividing the total number of births by the total number of fertile women aged 15–49 multiplied by 35, the total age-range.

Additionally to the child-level characteristics, the birth certificates provide information about the mother such as her age, marital status, citizenship, municipality, and canton of residence. The latter variable is important for identifying the causal effect as birth allowances are paid per canton and thus only births with non-missing information on the canton of residence of the mother are included in the analysis.

*Stillbirths and Deaths.* For the determination of more severe health outcomes, we rely on information provided in the statistics of stillbirths and deaths. As in the birth register, these two data sets provide information on the date, municipality, and canton of occurrence. For infants, the same maternal characteristics as before are recorded. Based on these two measures we calculate the stillbirth and infant ( $< 1$  year) death rate per 1,000 births.

*Birth Allowances.* The full history of birth allowances per canton are recorded in several publications. From 1969 to 1992 the data were published in *Zeitschrift für die Ausgleichs-kassen*. The publication *AHI-Praxis* covered the period from 1993 to 2004. Starting from 2005, the data are published online on the website of the FSIO. These publications record information on the date of implementation and the amount of the allowance per canton. Additionally, to the date of implementation also the date of announcement is recorded. All health policy reforms were announced no more than seven months prior to their implementation. This guarantees that around the implementation date the only adjustable margin is birth scheduling and no fertility adjustment as mothers were already pregnant by that time. In the long run, however, fertility can be affected — which we will analyze.



### 3.3.2 Descriptive Statistics

Descriptive statistics are shown in Table 3.1. Column 1 summarizes birth measures, child characteristics, and maternal characteristics for the overall sample. Column 2 focuses on cantons which introduced birth allowances at some point in time. Column 3 shows descriptive statistics for the rest of the cantons which serve as control cantons. For none of the reported measures there is a significant difference among the treated and control cantons. The sample of control cantons is, however, bigger than the one of treated cantons.

On average over the entire time period from 1969 to 2017, we observe around 81,000 births per year. The crude birth rate per 1,000 people in Switzerland is 11.8 and the total fertility rate per woman 1.6. On average, 5.0 fetuses out of 1,000 births die in a mother's womb and 7.6 infants out of 1,000 births die within the first year. At birth there are slightly more males (0.514) which directly translates into a sex ratio of 0.946 girls per 1 boy. The average Swiss family has a birth interval of slightly more than 3 years between children and the average birth weight of a newborn is 3,334 grams and the average length 50 cm. Mothers are on average 29 years old when giving birth, mostly married (91 percent) and 74 percent of them are Swiss.

Figure 3.1 shows the geographic variation in birth allowances for six different years. Cantons with birth allowances are mostly concentrated in the French speaking part and in the region of Central Switzerland. Certainly these treated cantons differ in various dimensions not covered in the birth measures from the control cantons. However, if it is the case that birth payments are introduced as a response to declining fertility, this would work against finding a positive result and thus our estimates would constitute lower bounds.

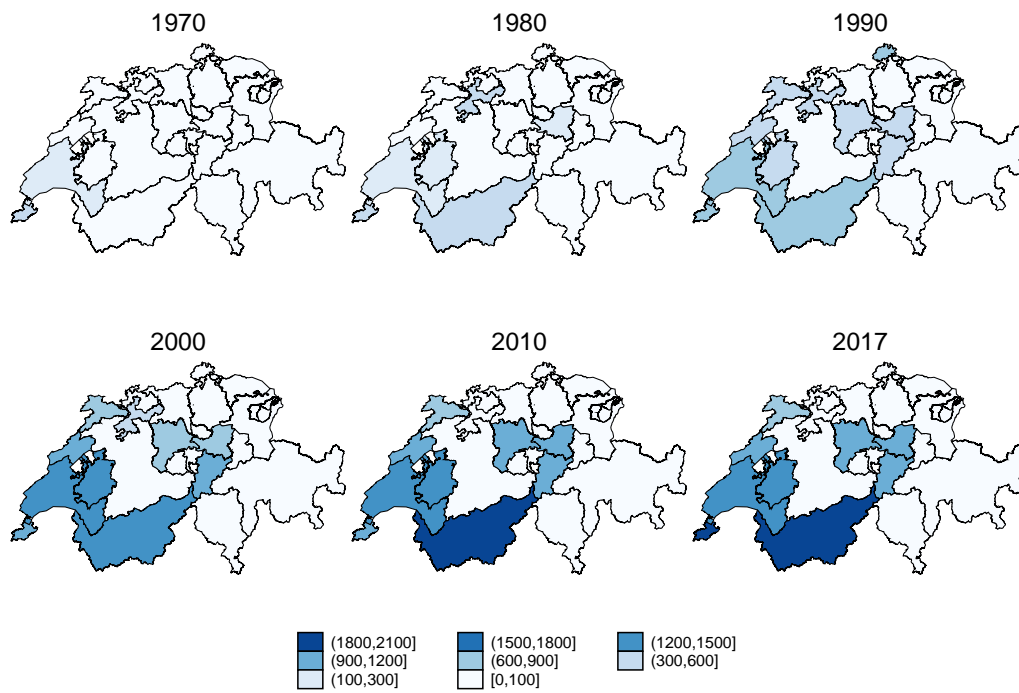
Figure 3.2 shows the time variation in birth allowances for all cantons that introduced the baby bonus at some point in time. Three cantons (Geneva, Vaud, and Fribourg) have

Table 3.1: Descriptive Statistics

	Full sample	Treated cantons	Control cantons
<b>Overall birth measures</b>			
Total births	80,578	29,766	50,812
(per year)	(7,083)	(2,718)	(4,727)
Crude birth rate	11.762	11.861	11.689
(per 1,000 people)	(2.170)	(1.963)	(2.308)
Total fertility rate	1.621	1.621	1.621
	(0.256)	(0.180)	(0.299)
Stillbirth rate	5.083	5.079	5.086
(per 1,000 births)	(2.841)	(2.421)	(3.115)
Infant death rate	7.581	7.665	7.520
(per 1,000 births)	(4.939)	(4.785)	(5.051)
<b>Child characteristics</b>			
Male	0.514	0.513	0.514
Birth interval	37.550	38.311	36.992
(in months)	(3.102)	(2.913)	(3.121)
Birth weight	3,333.551	3,319.076	3,344.166
	(49.468)	(56.009)	(40.974)
Length	49.628	49.512	49.713
	(0.441)	(0.438)	(0.424)
<b>Maternal characteristics</b>			
Age of the mother	28.985	28.868	29.070
	(1.586)	(1.576)	(1.589)
Married at birth	0.905	0.902	0.907
Swiss at birth	0.735	0.730	0.740
Observations	1,274	539	735

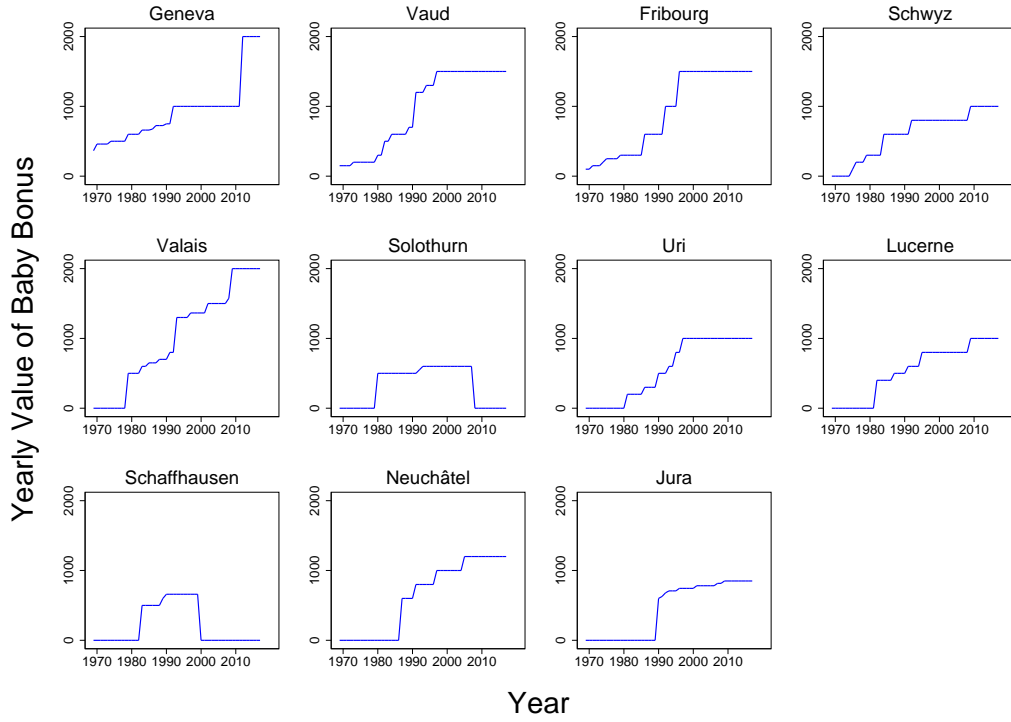
*Notes:* The full sample covers all births from 1969 to 2017. The treated cantons summarize all cantons which ever implement a birth allowance as shown in column 2. These cantons are Fribourg, Geneva, Jura, Lucerne, Neuchatel, Schaffhausen, Schwyz, Solothurn, Uri, Valais, and Vaud. All other cantons are considered as control cantons and summarized in column 3. Standard errors are shown in parentheses if necessary.

Figure 3.1: Geographic Variation of Birth Allowances



*Notes:* This Figure shows the amount of birth allowances provided per child per canton in current year values. The focus is on geographical variation so that birth allowances are drawn for all cantons every 10 years up to the most recent year available.

Figure 3.2: Time Variation of Birth Allowances by Treated Cantons



*Notes:* This Figure shows the amount of birth allowances provided per child per canton in current year values. It only shows the evolvement over time for those cantons which ever introduced a baby bonus at one point in time. The ordering of the cantons is according to their introduction year of the birth allowance.

already put baby allowances in place before 1969.<sup>7</sup> Several cantons adjust the amount of baby allowances over time. Some of these increases adjust for inflation, which is why in our estimations we will include payments in 2015 Swiss francs (CHF).<sup>8</sup> Other adjustments, however, are unrelated to inflation and increase the prevailing amount substantially. Two cantons (Solothurn and Schaffhausen) abolish the baby bonus after some years again.

<sup>7</sup>As the data set on the outcome measures only starts in 1969, the variation before 1969 cannot be exploited in this paper and is therefore not shown in the graphs.

<sup>8</sup>This transformation is based on the Swiss Consumer Price Index (CPI) provided by the Federal Statistical Office.

### 3.4 Empirical Strategy

Our baseline estimation strategy for the analysis of birth allowances on fertility and newborn health is a difference-in-differences regression where we study both the introduction and the intensity of the treatment.

With the following baseline model, we aim at identifying the causal effect of birth allowances on fertility and newborn health:

$$y_{ct} = \alpha D_{ct} + \omega'_{ct}\beta + \gamma_t + \zeta_c + \delta_c * t + \theta_c * t^2 + \epsilon_{ct} \quad (3.1)$$

The dependent variable,  $y_{ct}$  represents the log of the total number of births, the total fertility rate, the crude birth rate, birth weight, the birth interval in months, the sex ratio, the stillbirth rate, or the infant death rate in canton  $c$  and year  $t$ .  $D_{ct}$  is either a binary indicator for being under the regime of a baby bonus or an integer representing the actual value (in 2015 Swiss francs) of the baby allowances in order to also study the intensity of the treatment.<sup>9</sup> Thus,  $\alpha$  represents the estimated treatment effect of birth allowances on the outcome variable.  $\omega_{ct}$  is a vector of control variables to control for time-varying confounding canton-level characteristics such as the average maternal age, marital status, citizenship at birth, and the average value of the monthly child allowance (in 2015 Swiss francs). Year fixed effects and canton fixed effects are denoted by  $\gamma_t$  and  $\zeta_c$ .  $\delta_c$  and  $\theta_c$  capture linear and quadratic canton-specific time trends. Finally,  $\epsilon_{ct}$  is an error term. All estimates are weighted by number of births in the canton-year cell and standard errors are clustered by canton.

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<sup>9</sup>In an additional specification, we also include a quadratic term for the birth allowance.

### 3.5 Fertility and Newborn Health Results

Our baseline results are reported in Tables 3.2–3.3. We show estimates for the different set of outcome measures (fertility in Table 3.2 and newborn health in Table 3.3) and three specifications. The first line shows the results for the simple dummy specification indicating the effect of introducing the treatment. The second and third line report results for the intensity of the treatment. We show each a linear and a quadratic specification for the value (divided by 100) of the birth allowance.

The outcomes in Table 3.2 represent fertility measures. None of the estimates are significant, but all clearly show a positive sign. This points toward a positive fertility effect of the birth allowance. However, due to large standard errors we cannot reject that there is no effect.

The outcomes in Table 3.3 study the effect on several newborn health measures. The last two represent two severe and negative newborn health outcomes: a stillbirth and an infant death. A negative sign of the estimate is thus a positive impact of the birth allowance as it means a reduction in one of the two tragic events. For both the stillbirth rate and the infant death rate we find a negative sign of the estimate, which is highly significant in all three specifications for the stillbirth rate. Furthermore, the effect is relatively large. For the introduction of the birth allowance, we find a reduction in the stillbirth rate of 1.182. Interpreting this effect at the mean, this translates into a reduction of 23 percent. Also, the coefficient on an increase of the birth allowance per CHF 100 leads to a reduction in the stillbirth rate of 0.110. This corresponds to a 2 percent decline at the mean. Clearly, as is shown by the quadratic specification estimates, returns to an additional CHF 100 are declining.

In line with the reduction of the two negative health measures, Table 3.3 reports a positive and significant health impact on birth weight. While the baby bonus increases

Table 3.2: Main Estimation Results: Fertility Outcomes

Dependent variable	Log of total births	Total fertility rate	Crude birth rate
<b>Dummy specification</b>			
Birth allowance (dummy)=1	0.0336 (0.0291)	0.0302 (0.0324)	0.310 (0.334)
<b>Linear value specification</b>			
Birth allowance (value/100)	0.00213 (0.00340)	-0.00115 (0.00418)	0.0325 (0.0391)
<b>Quadratic value specification</b>			
Birth allowance (value/100)	0.00970 (0.00649)	0.00799 (0.00665)	0.100 (0.0720)
Birth allowance (quadratic)	-0.000416 (0.000324)	-0.000419 (0.000290)	-0.00371 (0.00360)
Mean of Dep. Var.	7.515	1.621	11.762
Observations	1,263	962	1,213

*Notes:* This Table is estimated on the full sample of control and treated cantons. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the canton-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\*. We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying canton-level characteristics, year and canton fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

birth weight by 21 grams, this, however, corresponds to a relatively small impact of 0.6 percent at the mean. Neither the sex ratio nor the birth interval in months from one birth to another are significantly affected.

In a next step we show results for the specification where we only include ever treated cantons in Tables 3.4–3.5. In this specification the identification solely relies on variation in the timing of the policy changes. Qualitatively the results are very much in line with our baseline specification where we include all cantons. However, compared to the previous results the positive effect of birth allowances on the log of total births and the crude birth rate turn significant. The point estimate for the log of total births is 0.061 and for the crude birth rate 0.563. The first estimate can directly be interpreted as a 6.1 percent

Table 3.3: Main Estimation Results: Newborn Health Outcomes

Dependent variable	Sex ratio	Interval (in months)	Birth weight	Stillbirth rate	Infant death rate
<b>Dummy specification</b>					
Birth allowance (dummy)=1	-0.0144 (0.0141)	-0.238 (0.361)	20.60** (8.238)	-1.182*** (0.311)	-0.214 (0.515)
<b>Linear value specification</b>					
Birth allowance (value/100)	0.000383 (0.00103)	0.0263 (0.0526)	-0.219 (1.015)	-0.110** (0.0487)	-0.0308 (0.0524)
<b>Quadratic value specification</b>					
Birth allowance (value/100)	-0.00208 (0.00223)	-0.0747 (0.0596)	4.313*** (1.476)	-0.283*** (0.0540)	-0.0607 (0.0983)
Birth allowance (quadratic)	0.000135 (0.000113)	0.00501 (0.00376)	-0.225*** (0.0542)	0.00952** (0.00428)	0.00164 (0.00576)
Mean of Dep. Var.	0.947	37.550	3333.551	5.083	7.581
Observations	1,263	1,013	1,013	1,263	1,263

*Notes:* This Table is estimated on the full sample of control and treated cantons. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the canton-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\*/. We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying canton-level characteristics, year and canton fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.



Table 3.4: Including Only Ever Treated Cantons: Fertility Outcomes

Dependent variable	Log of total births	Total fertility rate	Crude birth rate
<b>Dummy specification</b>			
Birth allowance (dummy)=1	0.0610*** (0.0190)	0.00373 (0.0314)	0.563** (0.225)
<b>Linear value specification</b>			
Birth allowance (value/100)	0.00216 (0.00322)	-0.00608 (0.00494)	0.0253 (0.0367)
<b>Quadratic value specification</b>			
Birth allowance (value/100)	0.0132** (0.00434)	0.00154 (0.00565)	0.127** (0.0518)
Birth allowance (quadratic)	-0.000614** (0.000229)	-0.000358 (0.000409)	-0.00570* (0.00290)
Mean of Dep. Var.	7.609	1.621	11.861
Observations	528	407	508

*Notes:* This Table is estimated solely on ever treated cantons. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the canton-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\*. We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying canton-level characteristics, year and canton fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

increase, while the second estimate evaluated at the mean shows a 4.7 percent increase in the crude birth rate due to the introduction of the baby bonus. Furthermore, the first CHF 100 increase both outcomes by roughly 1 percent. Similarly to the baseline specification, the effect on stillbirths is significant and negative. The point estimate of -0.888 is slightly smaller, but still refers to a 17.5 percent decline at the mean in the overall stillbirth rate. Also the positive impact of 15.28 grams on birth weight is in line with the previously reported results.

Finally, as we are interested in the effect on both the extensive and the intensive margin, we narrow our analysis and focus solely on the crude birth rate. We choose this variable as it is available for a longer time horizon than the total fertility rate and it still

Table 3.5: Including Only Ever Treated Cantons: Newborn Health Outcomes

Dependent variable	Sex ratio	Interval (in months)	Birth weight	Stillbirth rate	Infant death rate
<b>Dummy specification</b>					
Birth allowance (dummy)=1	-0.0159 (0.0181)	-0.469 (0.331)	15.28** (5.771)	-0.888** (0.377)	-0.615 (0.583)
<b>Linear value specification</b>					
Birth allowance (value/100)	0.000201 (0.00165)	0.0149 (0.0632)	-0.358 (1.085)	-0.0579 (0.0522)	-0.0527 (0.0402)
<b>Quadratic value specification</b>					
Birth allowance (value/100)	-0.00253 (0.00320)	-0.123* (0.0622)	3.871*** (0.906)	-0.213*** (0.0539)	-0.129 (0.100)
Birth allowance (quadratic)	0.000152 (0.000146)	0.00710 (0.00449)	-0.218*** (0.0422)	0.00862** (0.00286)	0.00425 (0.00526)
Mean of Dep. Var.	0.949	38.311	3319.076	5.079	7.665
Observations	528	428	428	528	528

*Notes:* This Table is estimated solely on ever treated cantons. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the canton-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\* . We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying canton-level characteristics, year and canton fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

Table 3.6: Including Only Ever Treated Cantons: Birth Order Analysis

Dependent variable	Crude birth rate (cbr)	1st-child cbr	2nd-child cbr	3d-child cbr
<b>Dummy specification</b>				
Birth allowance (dummy)=1	0.563** (0.225)	0.265** (0.115)	0.210** (0.0730)	0.119** (0.0416)
<b>Linear value specification</b>				
Birth allowance (value/100)	0.0253 (0.0367)	0.0185 (0.0152)	0.0109 (0.0119)	0.00196 (0.00764)
<b>Quadratic value specification</b>				
Birth allowance (value/100)	0.127** (0.0518)	0.0527* (0.0246)	0.0458** (0.0174)	0.0280*** (0.00856)
Birth allowance (quadratic)	-0.00570* (0.00290)	-0.00192 (0.00116)	-0.00196 (0.00109)	-0.00147** (0.000565)
Mean of Dep. Var.	11.861	4.677	4.022	1.462
Observations	508	508	508	508

*Notes:* This Table is estimated solely on ever treated cantons. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the canton-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\*. We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying canton-level characteristics, year and canton fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

allows us to study the impact by birth order which we report in Table 3.6. Column 1 in Table 3.6 replicates column 3 from Table 3.4. Columns 2–4 of Table 3.6 create new outcome measures taking into account the birth order so that, for example, column 2 reports the effect on first births, i.e. the extensive margin, while columns 3–4 show the effect on the intensive margin.

Interestingly, the effect is positive and significant for all outcomes. Point estimates on first, second, and third crude birth rates are 0.265, 0.210, and 0.119, respectively. Interpreted at the mean, these estimates correspond to an increase of 5.6 percent, 5.2 percent, and 8.1 percent. This shows that both the extensive and the intensive margin are affected. In relative terms, however, it is especially the intensive margin that reacts.

*Robustness.* We conduct several robustness analyses. Results are reported in the Appendix. Here, we briefly describe the general findings in what follows.

Tables C.2–C.3 in the Appendix<sup>10</sup> show results for the specification where we exclude cantons that already had a baby bonus in place before the onset of data availability. This is to ensure that the trends in fertility in these cantons are not driving the results. Thus, in this specification we exclude the cantons of Geneva, Vaud, and Fribourg. Results are very similar both qualitatively and quantitatively to the baseline specification.

Additionally, to estimating everything on the canton-year level, we also conduct sensitivity analyses by estimating everything at the municipality-year level. These results are reported in Tables C.4–C.5. The variation in the treatment is solely coming from the cantonal level, so that — unsurprisingly — the results are similar. However, due to the increase in sample size we gain significance for the effect on the log of births and the crude birth rate in the linear value specification.

Due to the differential treatment time and various intensity treatment changes over time, it is not straightforward to test for parallel trends. To address this issue we also report estimates for including  $D_{c(t+1)}$  in Equation (3.1). Conditional on including the current value effect  $D_{ct}$ ,  $D_{c(t+1)}$  should not contain any significant information, which is confirmed and reported in Tables C.6–C.7.

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<sup>10</sup>All following Figures and Tables denoted by C are reported in the *Appendix: Chapter 3*.

## 3.6 Birth Scheduling Results

In Figure 3.3 we report the graphical results of our birth scheduling analysis. They are represented in an event study design where we first collapse our individual level data on the daily level and then regress the following equation:

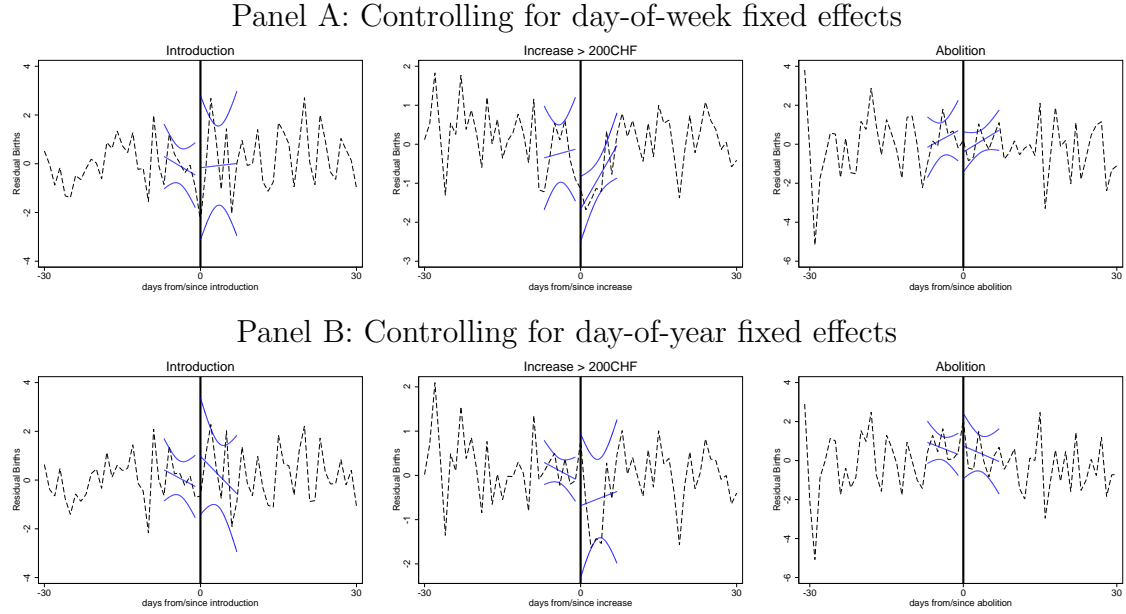
$$Y_{imyc} = \alpha + \beta_c + \gamma_y + \delta_m + \zeta_i + \epsilon_{imyc}, \quad (3.2)$$

where  $Y_{imyc}$  is the total (log) count of births per day  $i$ , in month  $m$ , in year  $y$ , and canton  $c$ . With  $\beta_c$  we include canton fixed effects, and with  $\gamma_y$  and  $\delta_m$  year and month fixed effects, respectively.  $\zeta_i$  are, depending on the specification, day-of-week or day-of-year fixed effects. Based on this, we calculate residuals from a linear prediction and plot these residuals for the 60 days around the policy change. We show the effect individually for introductions, increases of above CHF 200, and the abolition of the baby bonus where we pool over the same event across cantons and time.

For none of the three events (introduction, increase, and abolition), there is a clear pattern around the policy change. This holds true for both specifications reported in Panel A (day-of-week fixed effects) and Panel B (day-of-year fixed effects) and for both the total count of births per day as shown in Figure 3.3 and the log of the outcome variable as shown in Figure C.2. If anything, there is a slight decline in births after an increase in the baby bonus of more than CHF 200, which would contradict our expectations. However, due to the general noisy movement of daily births, this effect seems not systematic.

To understand the absence of birth scheduling in the Swiss case, which stands in contrast to the results in the literature, we try to understand the reasons. Therefore, we also check newspaper articles for media coverage of the baby bonus policy changes. For this we search for articles about birth allowances in LexisNexis and the archives of several

Figure 3.3: Birth Scheduling Event Study



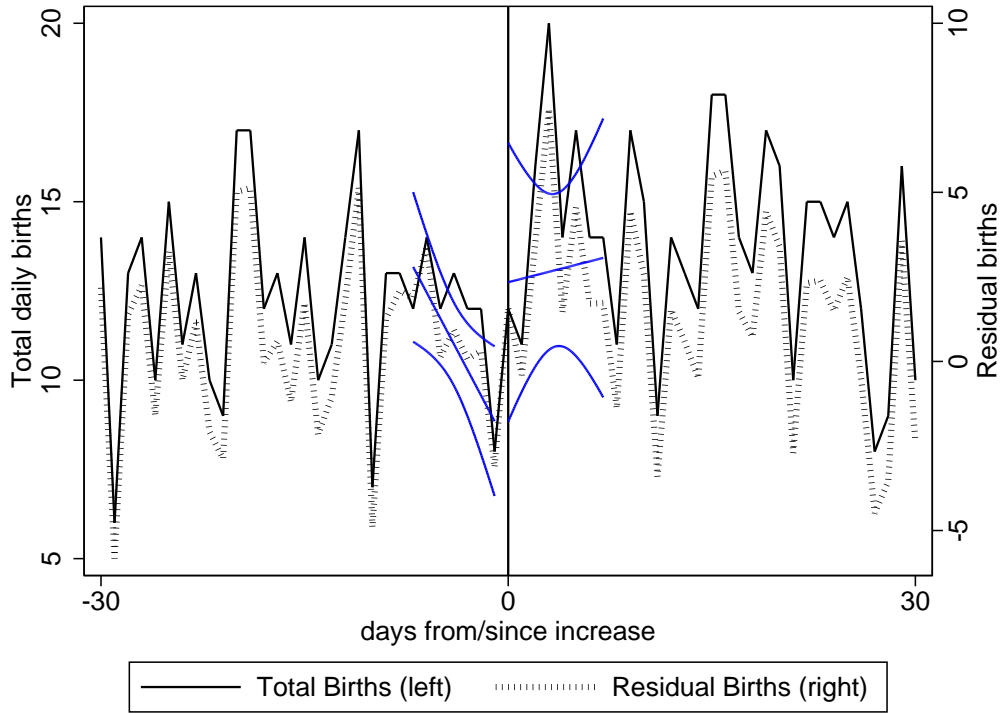
*Notes:* This Figure shows the residuals (in dashed black line) from a linear prediction of estimating Equation (3.2) of total births per day and a linear fit including a 95% confidence interval (in blue) for the week before and after the policy change. Panel A reports the residual when  $\zeta_i$  controls for day-of-week fixed effects and Panel B when  $\zeta_i$  controls for day-of-year fixed effects. The three event studies combine either all introductions, increases above CHF 200, or abolitions across cantons and time.

newspapers.<sup>11</sup> In general, media coverage is relatively low. This is especially true for increases of the baby bonus. Thus it is not surprising, that birth scheduling in general is not taking place.

Nevertheless, there is an exception. In 2012, the canton of Geneva doubled the amount paid from CHF 1,000 to CHF 2,000. This increase was the result of a cantonal referendum initiated by the political left. By the very nature of this policy change, this led to a lot of discussion and widespread information exchange. Thus, we decided to look in more detail at this specific increase, which is depicted in Figure 3.4. Both the total number of births and the residuals calculated as before show clearly, that there is an increase in births happening after the policy change. However, also for the specific case of Geneva, the baseline daily count of births is so low that in a standard regression analysis this effect

<sup>11</sup>These newspapers include *Tages Anzeiger*, *Neue Zürcher Zeitung*, *Blick*, *St. Galler Tagblatt*, *24heures*, and *Le Temps*.

Figure 3.4: Birth Scheduling Geneva: Policy Change on January 1st 2012



*Notes:* This Figure shows the daily count of total births (left axis) and the residuals (right axis) from a linear prediction of estimating Equation (3.2) of total births per day. Additionally, a linear fit is added including a 95% confidence interval (in blue) for the week before and after the policy change.  $\zeta_i$  controls for day-of-week fixed effects for the canton of Geneva. The 60 day window reports the 30 days pre- and post policy change (black vertical line) on January 1st 2012, where the baby bonus got doubled from CHF 1,000 to CHF 2,000 due to a public initiative.

does not reach significance.

Summarizing the analysis of birth scheduling, there seem to be only marginal behavioral effects of parents when it comes to rescheduling a birth. This is most likely the result of relatively low media coverage and small changes in the payment structure. In the specific case of Geneva in 2012, where parents were both informed and the baby bonus was doubled to CHF 2000, graphical analysis suggests a postponement of births into the new year. But also in this case, due to the small amount of daily births this effect is hard to significantly document in a consistent way.

### 3.7 Discussion

To discuss the robust and significant decline on the stillbirth rate, we want to provide some more background information on stillbirths, which are graphically depicted in Figure C.3.

Stillbirths have almost halved over the last 50 years, starting by slightly less than 10 stillbirths per 1,000 births in 1970 and moving downward to a bit above five stillbirths per 1,000 births nowadays. At the beginning of the described time period there were substantial differences among younger versus older mothers, foreign versus Swiss mothers, and unmarried versus married mothers. The gap between all of these groups has substantially decreased over time. Unfortunately, no other proxies for low-SES are available in the birth statistics.

The medical literature has identified various risk factors for a stillbirth (McClure et al., 2006, 2009; Bukowski R, 2011; Flenady et al., 2011; Silver, 2011; Varner et al., 2014). These factors can broadly be classified into four categories: (i) maternal characteristics, (ii) maternal medical conditions, (iii) maternal reproductive history, and (iv) fetal characteristics. Among (i) maternal characteristics, low-SES is one of the predominant drivers for a stillbirth and among (iii) maternal reproductive history, a first birth is more correlated with a stillbirth than a higher-order birth.

Both of these two stated drivers can explain the results we find. The financial support in form of a birth payment of up to CHF 2,000 may matter a lot for a low-SES mother. While raising a child up to age 20 is estimated to cost roughly CHF 200,000, a one percent transfer may seem negligible (NZZ, 2014). However, according to an estimation by Ming (2012) roughly CHF 3,500 are already spent on the newborn during pregnancy. Relative to these costs, the prospect of being paid CHF 2,000 upon arrival of the newborn may reduce stress during pregnancy tremendously and thus reduce the likelihood of a stillbirth. Furthermore, these CHF 2,000 may be especially important for low-SES mothers which



Table 3.7: Stillbirth Results: Heterogeneity Analysis

Dependent variable	Stillbirth rate	Stillbirth rate	Stillbirth rate	Stillbirth rate
<b>Dummy specification</b>				
Birth allowance (dummy)=1	-1.176*** (0.216)	-0.956*** (0.264)	-1.276*** (0.289)	-1.152*** (0.244)
Interaction above age average=1		-0.563*** (0.165)		
Interaction above Swiss average=1			0.463 (0.362)	
Interaction above married average=1				-0.0595 (0.176)
<b>Linear value specification</b>				
Birth allowance (value/100)	-0.112** (0.0464)	-0.0994** (0.0480)	-0.123** (0.0463)	-0.112** (0.0494)
Interaction above age average=1		-0.0394*** (0.0113)		
Interaction above Swiss average=1			0.0357* (0.0196)	
Interaction above married average=1				-0.000370 (0.0173)
Mean of Dep. Var	5.489	5.489	5.489	5.489
Observations	103,919	103,919	103,919	103,919

*Notes:* This Table is estimated on the full sample of control and treated cantons. The observational unit in this specification is at the municipal level. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the municipality-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\*. We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) and the interaction term with the stated heterogeneous group where we control for time-varying municipality-level characteristics, year and municipality fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

are less likely to be employed and thus not eligible for child allowances later on.

We support this argumentation by providing a heterogeneity analysis of the stillbirth results by characteristics of the mother, such as her age, her country of origin, and her marital status.<sup>12</sup> These results are shown in Table 3.7. For simplicity we only show the results for the dummy and the linear value specification. Both highlight that older mothers and those with a foreign background benefit more from birth allowances. There is no significant difference by marital status. Nevertheless, these results support the hypothesis that it is the mothers with lower socioeconomic background that gain significantly more from the introduction and higher payment of the baby bonus.

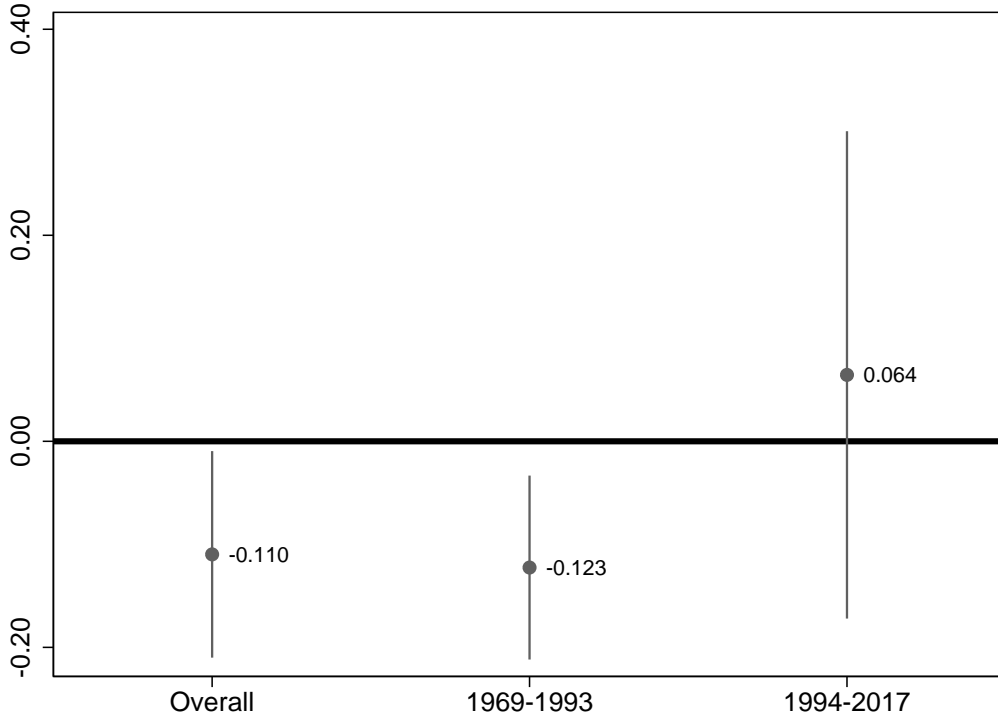
Additionally to this first mechanism, we also find a relative stronger fertility impact on higher-order births as shown in Table 3.6, which face a lower risk of stillbirth. These two factors combined may explain the very large and significant effect on the stillbirth rate we document across all our specifications.

Furthermore we are interested in the evolution of the effect over time. Due to the sample size and the structure of the policy changes (i.e. introductions of baby bonus take place relatively early) we concentrate on the linear value specification and split the sample in half to show an estimate for the period 1969–1993 and another one for 1994–2017. These results are shown in Figure 3.5. They show that the significant reduction in the stillbirth rate by 0.110 for an additional CHF 100 are entirely driven by the first period ranging from 1969–1993. Thereafter the point estimate becomes positive and insignificant.

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<sup>12</sup>We also conduct the heterogeneity analysis of the crude birth rate with respect to the same subgroups, which is available upon request. We find that an additional CHF 100 increase the crude birth rate significantly more for younger and foreign mothers.

Figure 3.5: The Effect on the Stillbirth Rate Over Time: Linear Value Specification



*Notes:* This Figure shows the point estimate of birth allowances on the stillbirth rate including the 95% confidence interval. This corresponds to estimating the linear value specification of Equation (3.1). We show the estimate for the entire sample and then for the two periods when we split the sample.

### 3.8 Conclusion

We exploit a unique quasi-experimental setting in Switzerland that allows us to study the effect of birth allowances on fertility, newborn health, and birth scheduling. In Switzerland, cantons are free to implement birth allowances and may choose the value paid. This gives rise to a lot of cantonal variation which we use in a difference-in-differences setting where we can test in our baseline specification both for the introduction but also the intensity of the treatment. Based on administrative data on births, stillbirths, and infant deaths, we can then analyze various outcome measures — the crude birth rate, the fertility rate, and several newborn health measures. Furthermore, to study birth scheduling we employ a graphical event study analysis on the basis of daily birth counts. This allows us to understand, whether parents are willing to shift births due to financial incentives.

We do not find evidence for birth scheduling, i.e. parents do neither postpone nor shift forward a birth as a reaction to an introduction or abolition of a baby bonus. We argue, that this is the result of several features, such as in general only small changes in the amount paid, low media coverage of any of the changes, and a relatively small sample size on the cantonal daily level.

In the long-run, however, when studying the impact on fertility and newborn health, we provide evidence for a positive impact on both margins. We find an increase of the crude birth rate of around 3 percent for introducing birth payments and 0.3 percent for every additional CHF 100 paid. This effect becomes significant in the specification, where we include only ever treated cantons and thus solely rely on the variation in treatment timing. With a fertility rate of 1.6 — well below the replacement rate of 2.1 — this is an encouraging result for Switzerland.

Furthermore, we report a significant decline in the stillbirth rate and an increase in birth weight. While the latter effect is relatively small, the decline in the stillbirth rate is substantial. The introduction of the baby bonus leads to a significant decline of 23 percent evaluated at the mean. An additional CHF 100 reduce the stillbirth rate by 2 percent, though, the quadratic specification shows that this effect has diminishing marginal returns. Investigating this further, we find that the reduction in the stillbirth rate is particularly strong for older or foreign mothers, and that the effect is entirely driven by increases in the birth allowances in the first half of the observation period, i.e. 1969–1993.

This paper confirms that parents react to financial incentives by adjusting their fertility behavior. In addition to the previous literature on birth allowances, we also find that the stillbirth rate — a very severe and negative newborn health outcome — is strongly affected. This result is interesting for policy makers, as it suggests, that birth allowances may affect societies beyond the direct effect of fertility adjustments.

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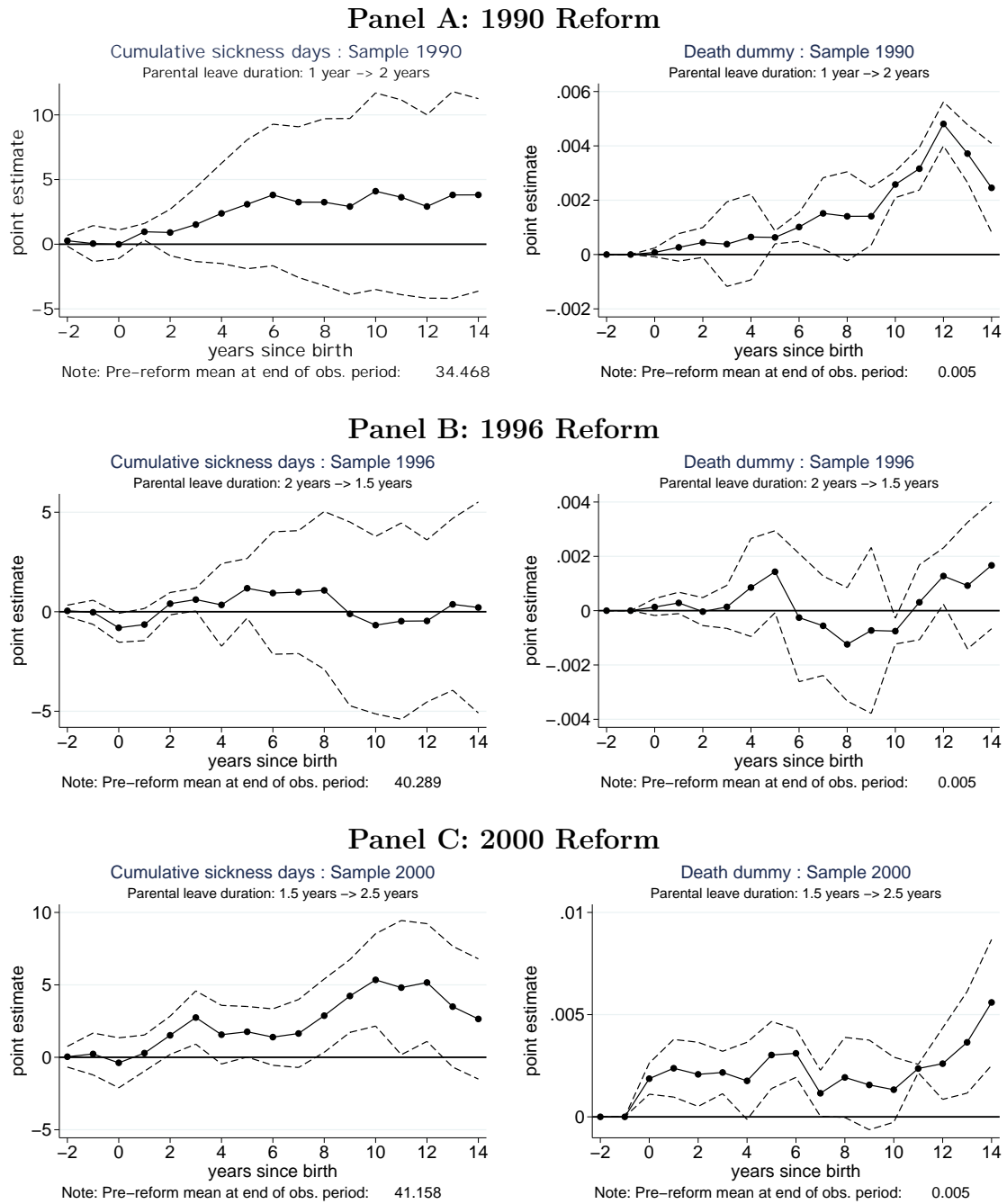
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# Appendix: Chapter 1

Figure A.1: Additional Health Outcomes Observable in ASSD



*Notes:* This Figure shows point estimates from estimating Equation (1.1) on accumulated health outcomes for the full population of working mothers. Each dot represents a separate estimation on accumulating the health outcome up to this point in time. Dashed lines report 95 percent confidence intervals.

Table A.1: Covariate Balance Test

Dependent variable	Maternal age at birth	Married	High-wage	Foreign origin
<b>1990 Reform</b>				
RDD-DiD	−0.0463 (0.0773)	−0.0319*** (0.00457)	0.0156 (0.0362)	0.0001 (0.0134)
Mean of Dep. Var.	26.180	0.742	0.549	0.056
Observations	13,313	13,313	13,313	13,313
<b>1996 Reform</b>				
RDD-DiD	0.262** (0.0771)	0.0420*** (0.00780)	0.0584*** (0.00965)	0.0287** (0.00998)
Mean of Dep. Var.	27.518	0.705	0.544	0.109
Observations	14,162	14,162	14,162	14,162
<b>2000 Reform</b>				
RDD-DiD	−0.192 (0.280)	0.0219 (0.0114)	−0.0246** (0.00665)	−0.00879 (0.0179)
Mean of Dep. Var.	28.362	0.655	0.562	0.123
Observations	12,383	12,383	12,383	12,383

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1). Each reform is individually estimated. The sample includes all mothers within a bandwidth of three months in a reform year and the preceding non-reform year, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable.

Table A.2: Robustness: Placebo Analysis

Dependent variable	Outpatient costs	GP costs	Medication costs	Days of hospitalizations
<b>1992 Reform</b>				
RDD-DiD	−215.8* (91.97)	−4.071*** (0.755)	−164.8 (109.2)	−1.057* (0.501)
Mean of Dep. Var.	2390.628	292.656	653.735	7.758
Observations	14,749	14,749	14,749	14,749
<b>1994 Reform</b>				
RDD-DiD	−23.46 (73.60)	−11.05 (11.90)	−38.84 (42.33)	−2.106 (1.250)
Mean of Dep. Var.	2319.359	289.359	606.257	7.979
Observations	15,179	15,179	15,179	15,179

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1). Each placebo reform is individually estimated. The sample includes all mothers within a bandwidth of three months in a placebo reform year and the preceding placebo non-reform year, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 15/13 years ago for the placebo reform in 1992/1994.

Table A.3: Estimates on General Health Outcomes: Including Covariates

Dependent variable	Outpatient costs	GP costs	Medication costs	Days of hospitalizations
<b>1990 Reform</b>				
RDD-DiD	−290.5* (135.1)	42.09 (35.56)	−336.1*** (71.78)	2.735** (0.818)
Mean of Dep. Var.	2422.887	314.097	665.960	8.227
Observations	13,313	13,313	13,313	13,313
<b>1996 Reform</b>				
RDD-DiD	−302.1 (168.7)	−15.79 (20.31)	−361.4* (163.0)	0.587 (0.774)
Mean of Dep. Var.	2371.067	271.462	631.638	7.589
Observations	14,162	14,162	14,162	14,162
<b>2000 Reform</b>				
RDD-DiD	262.2* (103.2)	−35.66*** (6.290)	175.1 (98.75)	−3.697** (1.218)
Mean of Dep. Var.	2354.290	257.068	424.966	15.126
Observations	12,383	12,383	12,383	12,383

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1) additionally controlling for maternal characteristics (age, marital status, origin, and high-wage). Each reform is individually estimated. The sample includes all mothers within a bandwidth of three months in a reform year and the preceding non-reform year, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 17/11/7 years ago for the reform in 1990/1996/2000.



Table A.4: Robustness of 1990 Reform: Bandwidth Choice

Bandwidth	3 months	4 months	5 months	6 months
<b>Dep. var. Outpatient costs</b>				
RDD-DiD	−290.6* (140.2)	−272.0** (107.3)	−84.70 (172.6)	−85.49 (137.6)
Mean of Dep. Var.	2422.887	2411.598	2446.584	2454.173
Observations	13,313	17,842	21,979	26,385
<b>Dep. var. GP costs</b>				
RDD-DiD	41.44 (36.45)	−11.59 (28.49)	27.37 (31.38)	13.40 (27.94)
Mean of Dep. Var.	314.097	299.047	304.724	307.861
Observations	13,313	17,842	21,979	26,385
<b>Dep. var. Medication costs</b>				
RDD-DiD	−337.4*** (70.27)	−292.1*** (75.81)	−142.1 (128.9)	−109.9 (107.4)
Mean of Dep. Var.	665.960	668.325	695.553	694.297
Observations	13,313	17,842	21,979	26,385
<b>Dep. var. Days of hospitalizations</b>				
RDD-DiD	2.764* (0.817)	1.130 (0.913)	1.853 (0.985)	1.696 (0.831)
Mean of Dep. Var.	8.227	7.883	7.928	8.125
Observations	13,313	17,842	21,979	26,385

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1). The sample includes all mothers within a bandwidth of 3–6 months as shown in Columns 1–4 in the reform year 1990 and the preceding non-reform year 1989, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 17 years ago.

Table A.5: Robustness of 1996 Reform: Bandwidth Choice

Bandwidth	3 months	4 months	5 months	6 months
<b>Dep. var. Outpatient costs</b>				
RDD-DiD	-275.3 (170.2)	6.808 (155.2)	4.488 (155.0)	-61.78 (162.7)
Mean of Dep. Var.	2371.067	2341.923	2322.856	2316.299
Observations	14,162	18,978	23,608	28,323
<b>Dep. var. GP costs</b>				
RDD-DiD	-11.02 (21.89)	7.504 (26.25)	2.940 (24.64)	-14.47 (23.74)
Mean of Dep. Var.	271.462	267.631	267.201	266.124
Observations	14,162	18,978	23,608	28,323
<b>Dep. var. Medication costs</b>				
RDD-DiD	-350.8* (163.0)	-163.8 (120.1)	-142.1 (125.1)	-165.7 (132.4)
Mean of Dep. Var.	631.638	616.824	598.491	596.071
Observations	14,162	18,978	23,608	28,323
<b>Dep. var. Days of hospitalizations</b>				
RDD-DiD	0.520 (0.765)	2.577 (1.534)	2.565 (1.294)	1.612 (1.042)
Mean of Dep. Var.	7.589	7.789	7.863	7.754
Observations	14,162	18,978	23,608	28,323

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1). The sample includes all mothers within a bandwidth of 3–6 months as shown in Columns 1–4 in the reform year 1996 and the preceding non-reform year 1995, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 11 years ago.

Table A.6: Robustness of 2000 Reform: Bandwidth Choice

Bandwidth	3 months	4 months	5 months	6 months
<b>Dep. var. Outpatient costs</b>				
RDD-DiD	255.1** (96.15)	189.6* (97.91)	327.0*** (82.00)	290.0*** (91.27)
Mean of Dep. Var.	2354.290	2384.973	2399.973	2405.945
Observations	12,383	16,423	20,212	24,179
<b>Dep. var. GP costs</b>				
RDD-DiD	-36.31*** (5.540)	-19.66 (10.60)	-11.01 (10.49)	-11.01 (9.310)
Mean of Dep. Var.	257.068	263.054	261.605	261.294
Observations	12,383	16,423	20,212	24,179
<b>Dep. var. Medication costs</b>				
RDD-DiD	171.6 (91.91)	203.2** (62.38)	259.5*** (59.69)	214.1*** (63.83)
Mean of Dep. Var.	424.966	439.364	458.809	468.719
Observations	12,383	16,423	20,212	24,179
<b>Dep. var. Days of hospitalizations</b>				
RDD-DiD	-3.718* (1.260)	-2.718* (0.935)	-1.063 (1.135)	-0.930 (0.991)
Mean of Dep. Var.	15.126	15.009	15.021	14.926
Observations	12,383	16,423	20,212	24,179

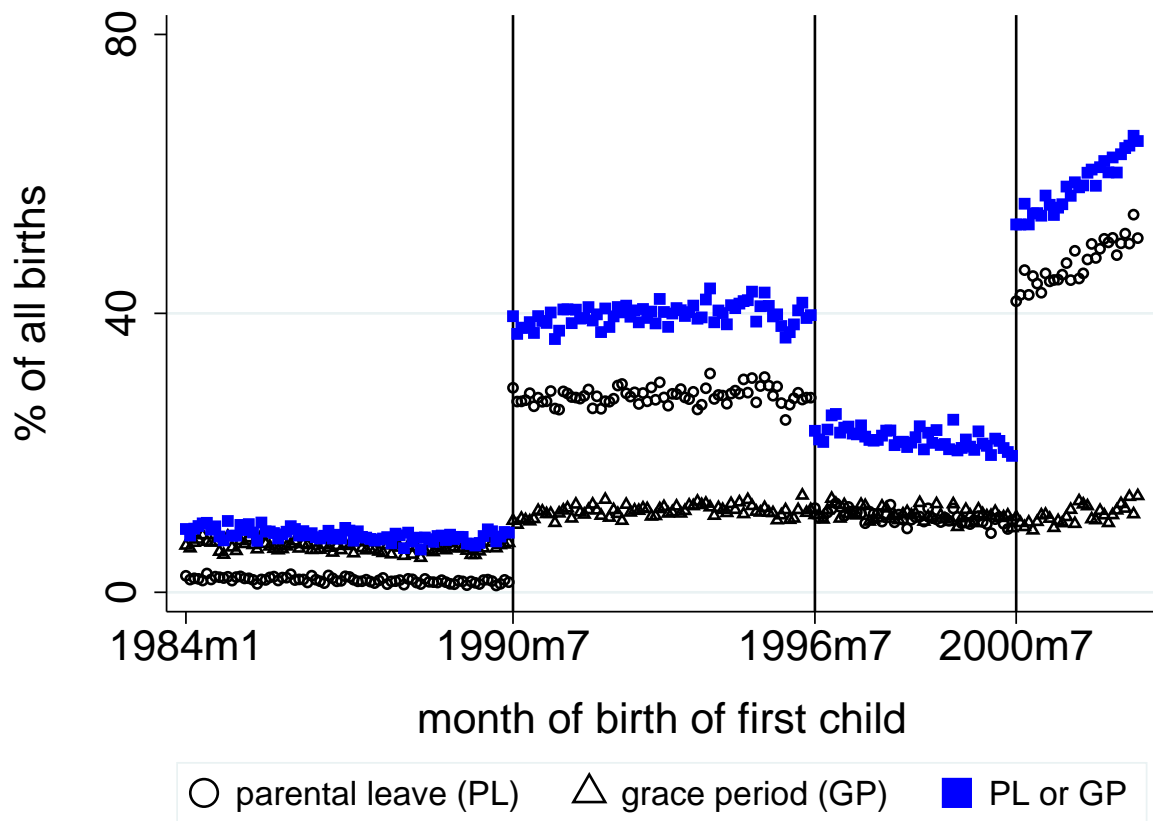
\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors in parentheses.

*Notes:* This Table provides parameter estimates for the coefficient  $\beta_1$  of Equation (1.1). The sample includes all mothers within a bandwidth of 3–6 months as shown in Columns 1–4 in the reform year 2000 and the preceding non-reform year 1999, to control for seasonality around the policy cutoff. Standard errors are clustered at the level of the running variable. Health outcomes are accumulated over time up to 2007, so that birth occurred 7 years ago.



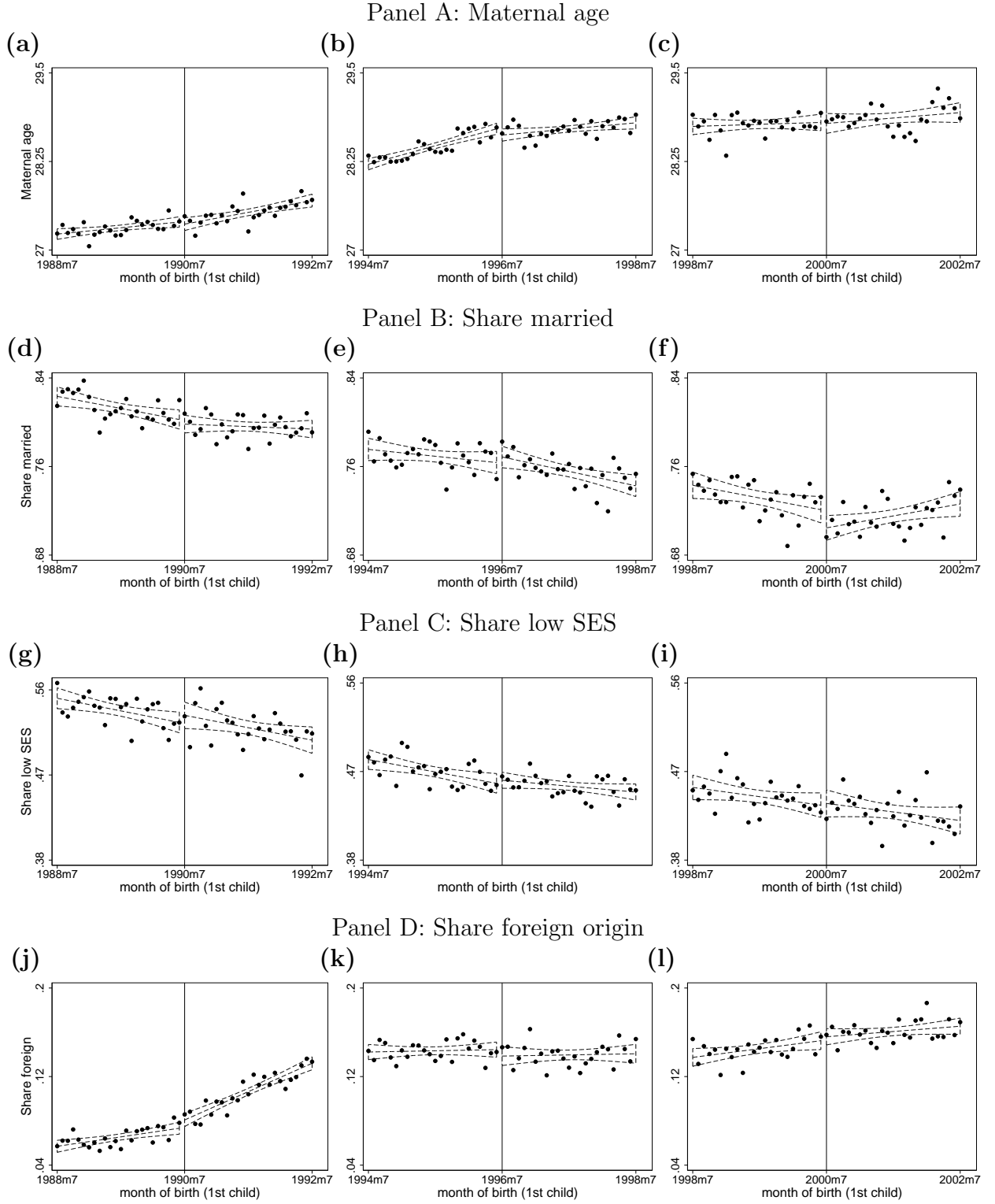
## Appendix: Chapter 2

Figure B.1: Second Children Born Within Parental Leave and Grace Period



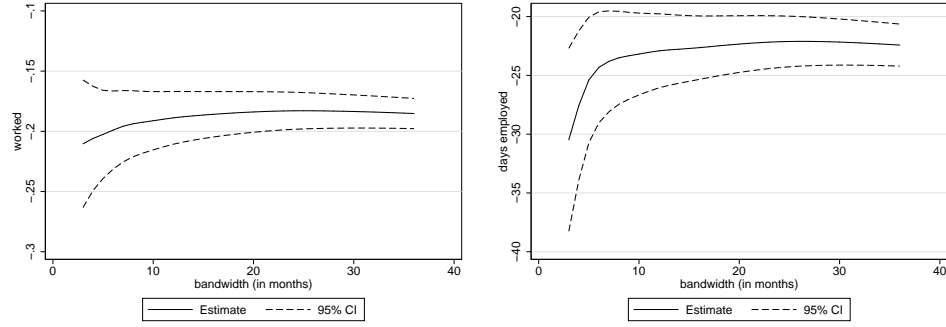
*Notes:* This Figure shows the fraction of first born in families with at least two children that have a younger sibling that is born within the parental leave and grace period. Dots refer to the percentage of second children born within the parental leave period of the first child. Triangles refer to the percentage of second children born within the grace period after the parental leave period of the first child. Squares refer to the sum of both. The solid vertical lines refer to the three policy changes, increasing the latest consecutive birth date to 27.5 months in July 1990, decreasing it to 21.5 months in July 1996 and raising it again to 33.5 months in July 2000. The data consists of the full matched and eligible sample of mothers with at least two children.

Figure B.2: RDD Plots Covariates

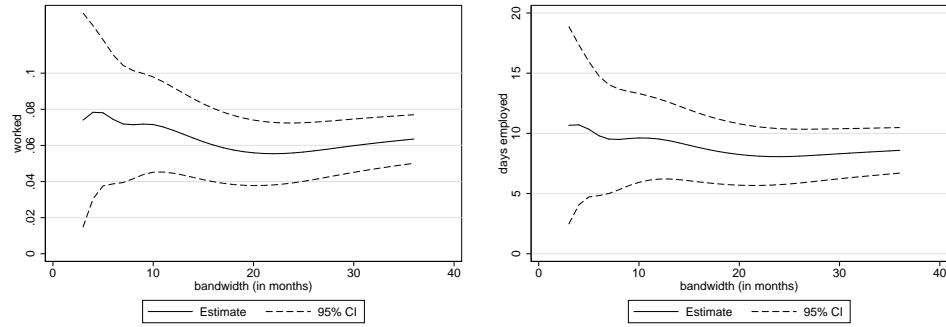


*Notes:* This Figure reports the composition of mothers giving birth to their second child by month of birth of the first child. All subfigures are based on the full sample of matched and eligible mothers that gave birth to their first child not more than 24 months apart from a policy reform.

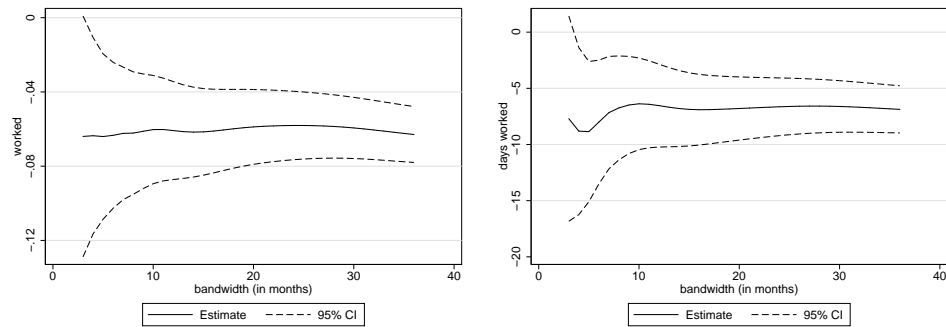
Figure B.3: Robustness to Different Choices of Bandwidths  
Panel A: 1990



Panel B: 1996

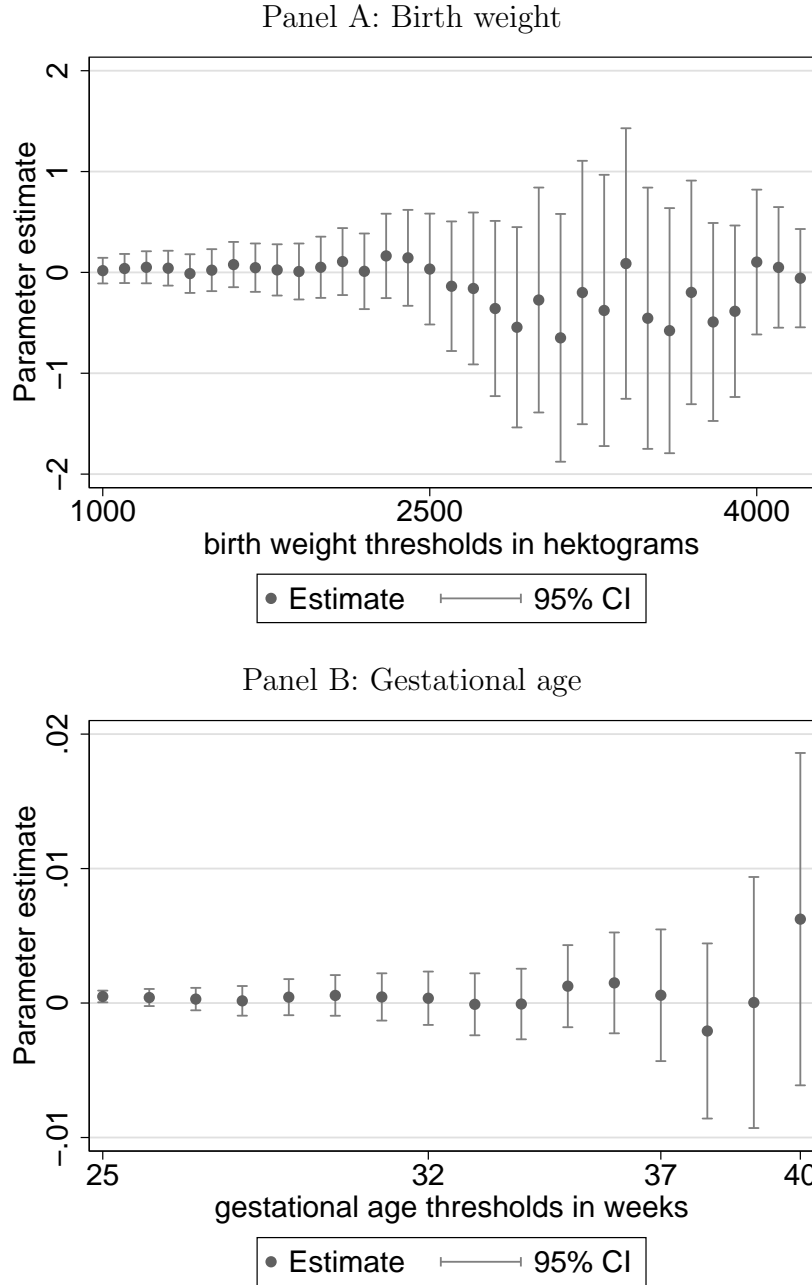


Panel C: 2000



*Notes:* This Figure reports the robustness of the RDD estimate on the work status and the days employed from Equation (2.2) to different choices of bandwidths. The solid line refers to the point estimate of separate equations, and the dashed lines indicate the corresponding 95 percent confidence interval. Panel A and C refer to the increase in parental leave duration in July 1990 and July 2000, respectively. Panel B shows the policy reform in July 1996, where parental leave duration declined. All Panels were estimated using the full sample of matched and eligible mothers.

Figure B.4: RDD Estimates for Different Thresholds: 1990 Reform



*Notes:* This Figure reports parameter estimates for the coefficient  $\delta_1$  in Equation (2.3). In Panel A (B) for every 100 grams (week) a separate regression is estimated, corresponding to another birth weight (gestational age) threshold than the one used in the paper of 2,500 grams (37 weeks). 95 percent confidence intervals are shown by vertical lines. All estimates are based on regressions including the full set of controls. The sample consists of all matched and eligible mothers that gave birth to their first child not more than 24 months apart from the policy reform in July 1990.



Table B.1: Covariate Balance Test

	1990	1996	2000
<b>Panel A: Dependent variable maternal age</b>			
1{Post policy reform}	-0.032 (0.059)	-0.096 (0.062)	-0.004 (0.072)
Comparison Mean	27.412	28.735	28.822
<b>Panel B: Dependent variable married</b>			
1{Post policy reform}	-0.003 (0.005)	0.006 (0.006)	-0.014** (0.007)
Comparison Mean	0.801	0.763	0.719
<b>Panel C: Dependent variable share low SES</b>			
1{Post policy reform}	0.008 (0.007)	0.005 (0.007)	0.001 (0.008)
Comparison Mean	0.522	0.456	0.432
<b>Panel D: Dependent variable share foreign origin</b>			
1{Post policy reform}	0.007* (0.004)	-0.006 (0.005)	0.003 (0.006)
Comparison Mean	0.073	0.144	0.152
Observations	87,566	77,279	63,481

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses.

*Notes:* This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth just after the policy reform versus just before.

Table B.2: OLS Results on Preterm and Low Birth Weight:  
Full Set of Controls

Dependent Variable	Preterm		Low birth weight	
Panel A: Work status				
Worked	−0.0020** (0.0008)	−0.0021*** (0.0008)	−0.0027*** (0.0008)	−0.0023*** (0.0008)
Sick	0.0322*** (0.0018)	0.0313*** (0.0018)	0.0314*** (0.0018)	0.0305*** (0.0018)
Married		−0.0056*** (0.0010)		−0.0094*** (0.0010)
Foreign		0.0047*** (0.0013)		−0.0003*** (0.0012)
Aged 20-24		−0.0185*** (0.0047)		−0.0184*** (0.0048)
Aged 25-29		−0.0202*** (0.0047)		−0.0214*** (0.0048)
Aged 30-34		−0.0149*** (0.0048)		−0.0173*** (0.0048)
Aged 35-39		−0.0027 (0.0050)		−0.0027 (0.0051)
Aged 40-45		0.0139* (0.0074)		0.0110 (0.0074)
Low SES		0.0034*** (0.0008)		0.0044*** (0.0008)

*Notes:* This Table continues on the next page.

Table B.2 continued: OLS Results on Preterm and Low Birth Weight:  
Full Set of Controls

Dependent Variable	Preterm		Low birth weight	
Panel B: Days worked				
Days worked	−0.0001*** (0.0000)	−0.0001*** (0.0000)	−0.0001*** (0.0000)	−0.0001*** (0.0000)
Sick	0.0312*** (0.0018)	0.0305*** (0.0018)	0.0305*** (0.0018)	0.0298*** (0.0018)
Married		−0.0054*** (0.0010)		−0.0092*** (0.0010)
Foreign		0.0045*** (0.0013)		−0.0005*** (0.0012)
Aged 20-24		−0.0176*** ((11.0632))		−0.0177*** (0.0048)
Aged 25-29		−0.0185*** (0.0047)		−0.0201*** (0.0048)
Aged 30-34		−0.0126*** (0.0048)		−0.0155*** (0.0048)
Aged 35-39		−0.0001 (0.0050)		−0.0008 (0.0051)
Aged 40-45		0.0165** (0.0074)		0.0130*** (0.0074)
Low SES		0.0032*** (0.0008)		0.0043*** (0.0008)
Mother Controls	No	Yes	No	Yes
Mean of Dep. Var.	0.0335	0.0335	0.0327	0.0327
Observations	226,824	226,824	226,824	226,824

\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses.

*Notes:* This Table is estimated on the pooled sample of the three RDD regression windows. Additional controls included in all columns are year and month of birth FE, and a gender dummy. Mother's characteristic controls are dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy.

Table B.3: Robustness to Different Functional Forms: 1990 Reform

Spline	Linear		Linear Interaction		Quadratic		Quadratic Interaction	
<b>Panel A: Dependent variable work status</b>								
1{Post policy reform}	-0.191*** (0.006)	-0.190*** (0.006)	-0.191*** (0.006)	-0.191*** (0.006)	-0.191*** (0.006)	-0.190*** (0.006)	-0.185*** (0.010)	-0.183*** (0.009)
Comparison Mean	0.713	0.713	0.714	0.714	0.712	0.712	0.716	0.716
<b>Panel B: Dependent variable days worked</b>								
1{Post policy reform}	-22.965*** (0.919)	-22.802*** (0.885)	-23.105*** (0.924)	-23.011*** (0.891)	-23.042*** (0.922)	-22.933*** (0.888)	-22.244*** (1.401)	-22.030*** (1.349)
Comparison Mean	82.104	82.104	82.720	82.720	82.400	82.400	82.825	82.825
Additional Mother Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	87,566	87,566	87,566	87,566	87,566	87,566	87,566	87,566

\* p < 0.1, \*\* p < 0.05, \*\*\* p < 0.001. Robust standard errors in parentheses.

*Notes:* This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. Additional mother's characteristic controls include dummies for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy. The 1{*post policy reform*} coefficient estimate reports the impact of a first birth on maternal employment just after the policy reform versus just before. The models that are tested are linear, linear interaction, quadratic, and quadratic interaction as described by Jacob et al. (2012).

Table B.4: Age Differences in Detail

	1990 sample	1996 sample	2000 sample
0–18 months	−0.040*** (0.005)	0.039*** (0.004)	−0.006 (0.005)
0–36 months	0.021*** (0.006)	0.002 (0.007)	−0.007 (0.008)
0–120 months	0.004* (0.002)	0.004*** (0.001)	
0–16 months	−0.030*** (0.004)		
17–28 months	0.057*** (0.006)		
29–120 months	−0.023*** (0.007)		
0–22 months		0.046*** (0.006)	
23–28 months		−0.050*** (0.005)	
29–120 months		0.009 (0.007)	
0–22 months			−0.031*** (0.007)
23–34 months			0.018** (0.007)
35–120 months			0.013* (0.008)
Observations	87,413	76,943	63,134

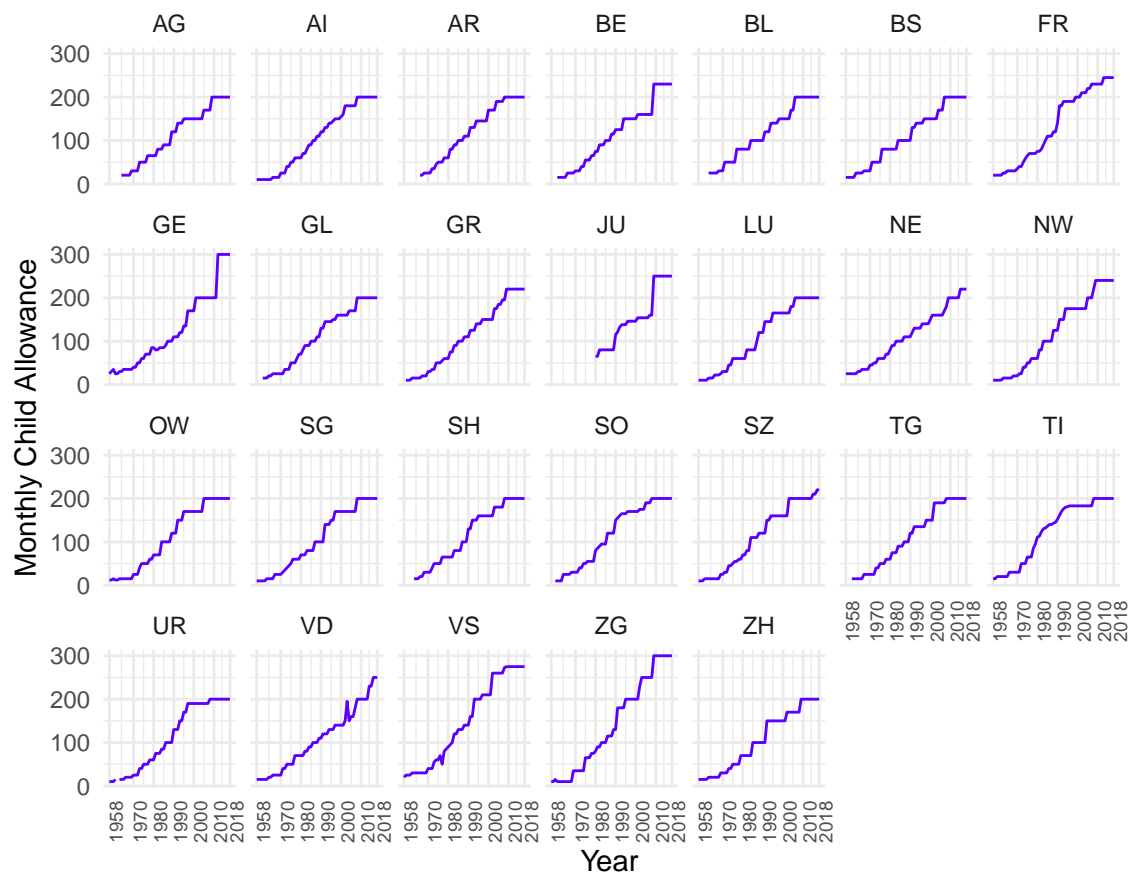
\*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.001$ . Robust standard errors in parentheses.

*Notes:* This Table is estimated on the matched and eligible sample of second born with an older sibling born within a bandwidth of 24 months around the policy reforms. This table reports the  $1\{post\ policy\ reform\}$  parameter estimate on the respective age difference between first and second born child. All columns include mother's characteristic controls for 5 year age brackets, marital status, a dummy for low SES (combined from educational and wage data) and a foreign origin dummy.



# Appendix: Chapter 3

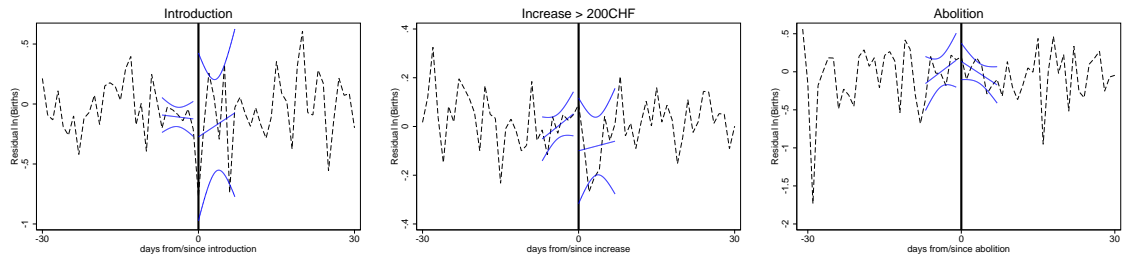
Figure C.1: Monthly Child Allowances per Canton



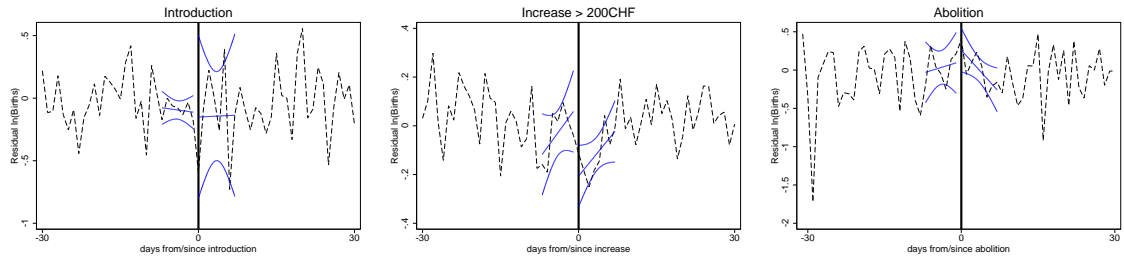
*Notes:* This Figure shows the amount of child allowances provided per child per month per canton in current year values.

Figure C.2: Birth Scheduling Log Specification Event Study

Panel A: Controlling for day-of-week fixed effects



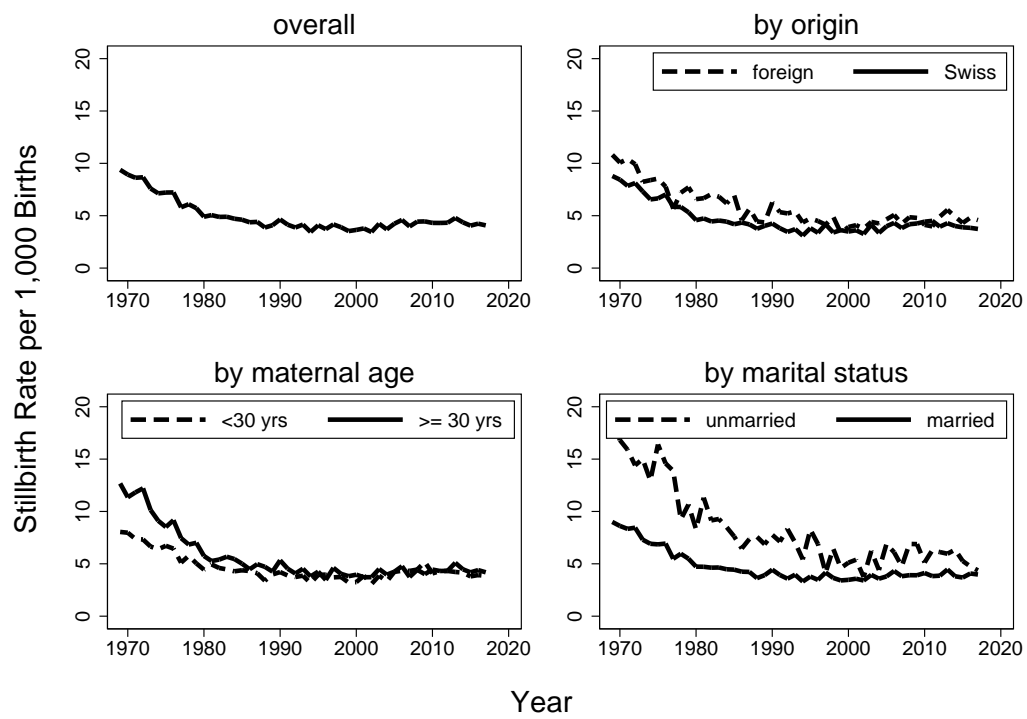
Panel B: Controlling for day-of-year fixed effects



*Notes:* This Figure shows the residuals (in dashed black line) from a linear prediction of estimating Equation (3.2) of the log of total births per day and a linear fit including a 95% confidence interval (in blue) for the week before and after the policy change. Panel A reports the residual when  $\zeta_i$  controls for day-of-week fixed effects and Panel B when  $\zeta_i$  controls for day-of-year fixed effects. The three event studies combine either all introductions, increases above CHF 200, or abolitions across cantons and time.



Figure C.3: Stillbirths in Switzerland



*Notes:* This Figure shows the stillbirth rate per 1,000 births once for the overall population, and then each by origin, maternal age, and maternal marital status.

Table C.1: Overview Policy Changes in Detail

Canton	Announcement Date	Implementation Date	Canton	Announcement Date	Implementation Date
Geneva	14.03.1969	01.05.1969	Valais	28.09.1990	01.01.1991
Fribourg	15.12.1970	01.01.1971	Vaud	30.11.1990	01.01.1991
Vaud	27.11.1972	01.01.1973	Neuchatel	03.12.1990	01.01.1991
Geneva	12.06.1973	01.07.1973	Jura	04.12.1990	01.01.1991
Fribourg	24.09.1973	01.01.1974	Geneva	12.12.1990	01.01.1991
Schwyz	09.05.1974	01.07.1974	Lucerne	18.12.1990	01.01.1991
Fribourg	29.10.1974	01.01.1975	Fribourg	18.02.1991	01.03.1991
Schwyz	05.12.1975	01.01.1976	Jura	16.04.1991	01.10.1991
Schwyz	25.09.1977	01.01.1978	Solothurn	15.10.1991	01.01.1992
Valais	01.12.1977	01.01.1978	Schwyz	08.12.1991	01.01.1992
Solothurn	12.06.1978	01.01.1979	Valais	06.04.1992	01.01.1993
Geneva	12.10.1978	01.01.1979	Jura	20.09.1992	01.01.1993
Fribourg	10.10.1978	01.07.1979	Solothurn	12.11.1992	01.01.1993
Vaud	18.09.1979	01.01.1980	Uri	08.12.1992	01.01.1993
Uri	28.09.1980	01.01.1981	Vaud	26.11.1993	01.01.1994
Lucerne	10.03.1980	01.07.1981	Lucerne	13.09.1994	01.01.1995
Vaud	13.11.1981	01.01.1982	Uri	28.09.1994	01.01.1995
Schaffhausen	24.06.1982	01.07.1982	Fribourg	13.11.1995	01.01.1996
Geneva	07.03.1982	01.07.1982	Jura	21.11.1995	01.01.1996
Valais	12.11.1982	01.01.1983	Valais	11.09.1996	01.01.1997
Schwyz	20.10.1983	01.01.1984	Vaud	24.09.1996	01.01.1997
Vaud	12.12.1983	01.01.1984	Uri	13.11.1996	01.01.1997
Valais	16.11.1984	01.01.1985	Neuchatel	27.11.1996	01.01.1997
Geneva	15.02.1985	01.04.1985	Schaffhausen	05.09.1999	01.01.2000
Fribourg	25.09.1985	01.01.1986	Jura	31.10.2000	01.01.2001
Uri	08.10.1985	01.01.1986	Valais	23.09.2001	01.01.2002
Geneva	25.06.1986	01.01.1987	Neuchatel	01.12.2004	01.01.2005
Neuchatel	20.10.1986	01.01.1987	Jura	26.11.2006	01.01.2007
Lucerne	14.11.1986	01.01.1987	Valais	31.10.2007	01.01.2008
Valais	13.11.1987	01.07.1988	Solothurn	16.11.2007	01.01.2008
Schaffhausen	06.06.1988	01.07.1988	Valais	11.09.2008	01.01.2009
Vaud	09.11.1988	01.01.1989	Schwyz	28.09.2008	01.01.2009
Jura	24.02.1989	01.07.1989	Jura	25.11.2008	01.01.2009
Uri	08.06.1989	01.01.1990	Lucerne	28.11.2008	01.01.2009
Geneva	27.09.1989	01.01.1990	Geneva	23.06.2011	01.01.2012
Schaffhausen	06.11.1989	01.01.1990			

*Notes:* This Table gives detailed information on every policy change regarding birth allowances in Switzerland starting in 1969 and informs about the announcement date and about the implementation date of each stated change.

Table C.2: Excluding Early Adopters: Fertility Outcomes

Dependent variable	Log of total births	Total fertility rate	Crude birth rate
<b>Dummy specification</b>			
Birth allowance (dummy)=1	0.0241 (0.0318)	0.0387 (0.0327)	0.198 (0.368)
<b>Linear value specification</b>			
Birth allowance (value/100)	0.00408 (0.00457)	0.00191 (0.00395)	0.0378 (0.0517)
<b>Quadratic value specification</b>			
Birth allowance (value/100)	0.00369 (0.00532)	0.00757 (0.00621)	0.0488 (0.0584)
Birth allowance (quadratic)	0.0000382 (0.000146)	-0.000424 (0.000269)	-0.00105 (0.00161)
Mean of Dep. Var.	7.399	1.621	11.739
Observations	1,116	851	1,072

*Notes:* This Table is estimated on the sample of control and treated cantons excluding those cantons (Geneva, Vaud, and Fribourg) which introduced the baby bonus before 1969. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the canton-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\*. We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying canton-level characteristics, year and canton fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

Table C.3: Excluding Early Adopters: Newborn Health Outcomes

Dependent variable	Sex ratio	Interval (in months)	Birth weight	Stillbirth rate	Infant death rate
<b>Dummy specification</b>					
Birth allowance (dummy)=1	-0.0169 (0.0148)	-0.105 (0.365)	21.52** (9.505)	-1.193*** (0.342)	-0.0132 (0.488)
<b>Linear value specification</b>					
Birth allowance (value/100)	-0.000866 (0.00150)	-0.0312 (0.0332)	1.605 (1.399)	-0.140** (0.0542)	-0.0130 (0.0497)
<b>Quadratic value specification</b>					
Birth allowance (value/100)	-0.00529* (0.00264)	-0.0355 (0.0678)	4.793** (1.890)	-0.351*** (0.0601)	-0.0892 (0.101)
Birth allowance (quadratic)	0.000427** (0.000173)	0.000364 (0.00480)	-0.269*** (0.0946)	0.0204*** (0.00529)	0.00736 (0.00655)
Mean of Dep. Var.	0.948	37.127	3339.766	5.108	7.584
Observations	1,116	896	896	1,116	1,116

*Notes:* This Table is estimated on the sample of control and treated cantons excluding those cantons (Geneva, Vaud, and Fribourg) which introduced the baby bonus before 1969. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the canton-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\* . We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying canton-level characteristics, year and canton fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

Table C.4: Municipality Level Specification: Fertility Outcomes

Dependent variable	Log of total births	Total fertility rate	Crude birth rate
<b>Dummy specification</b>			
Birth allowance (dummy)=1	0.0985 (0.0663)	0.0453 (0.0470)	0.686 (0.678)
<b>Linear value specification</b>			
Birth allowance (value/100)	0.0171*** (0.00513)	0.00333 (0.00206)	0.0530* (0.0293)
<b>Quadratic value specification</b>			
Birth allowance (value/100)	0.0289** (0.0139)	0.0145 (0.00992)	0.210 (0.140)
Birth allowance (quadratic)	-0.000631 (0.000522)	-0.000506 (0.000405)	-0.00710 (0.00566)
Mean of Dep. Var.	7.515	1.621	11.762
Observations	103,919	79,000	79,000

*Notes:* This Table is estimated on the full sample of control and treated cantons. The observational unit in this specification is at the municipal level. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the municipality-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\*. We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying municipality-level characteristics, year and municipality fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

Table C.5: Municipality Level Specification: Newborn Health Outcomes

Dependent variable	Sex ratio	Interval (in months)	Birth weight	Stillbirth rate	Infant death rate
<b>Dummy specification</b>					
Birth allowance (dummy)=1	-0.0120 (0.0128)	-0.322 (0.203)	19.67*** (3.167)	-1.176*** (0.216)	-0.146 (0.549)
<b>Linear value specification</b>					
Birth allowance (value/100)	-0.000214 (0.000912)	0.0328 (0.0433)	-0.239 (0.818)	-0.112** (0.0464)	0.0105 (0.0589)
<b>Quadratic value specification</b>					
Birth allowance (value/100)	-0.00171 (0.00204)	-0.0845 (0.0540)	4.592*** (0.640)	-0.255*** (0.0663)	0.0248 (0.111)
Birth allowance (quadratic)	0.0000802 (0.0000796)	0.00571** (0.00254)	-0.235*** (0.0303)	0.00765 (0.00512)	-0.000763 (0.00543)
Mean of Dep. Var.	0.947	37.550	3333.551	5.083	7.581
Observations	100,270	80,454	83,223	103,919	103,919

*Notes:* This Table is estimated on the full sample of control and treated cantons. The observational unit in this specification is at the municipal level. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the municipality-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\*.

We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying municipality-level characteristics, year and municipality fixed effects, and linear and quadratic canton-specific time trends. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

Table C.6: Placebo Estimation: Fertility Outcomes

Dependent variable	Log of total births	Total fertility rate	Crude birth rate
<b>Linear value specification</b>			
Birth allowance (value/100)	0.00170 (0.00226)	-0.000862 (0.00217)	0.0274 (0.0271)
Birth allowance (t+1)	0.000421 (0.00265)	-0.00133 (0.00409)	0.00403 (0.0278)
Mean of Dep. Var.	7.515	1.621	11.762
Observations	1,237	936	1,187

*Notes:* This Table is estimated on the full sample of control and treated cantons. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the canton-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\*. We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying canton-level characteristics, year and canton fixed effects, linear and quadratic canton-specific time trends and additionally for  $D_{c(t+1)}$ , which we also report in this Table. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.

Table C.7: Placebo Estimation: Newborn Health Outcomes

Dependent variable	Sex ratio	Interval (in months)	Birth weight	Stillbirth rate	Infant death rate
<b>Linear value specification</b>					
Birth allowance (value/100)	0.000189 (0.00140)	0.000144 (0.0383)	0.412 (0.955)	-0.0915* (0.0487)	0.0127 (0.0799)
Birth allowance (t+1)	0.000398 (0.00128)	0.0463 (0.0479)	-1.082* (0.606)	-0.0194 (0.0565)	-0.0549 (0.0610)
Mean of Dep. Var.	0.947	37.550	3333.551	5.083	7.581
Observations	1,237	987	987	1,237	1,237

*Notes:* This Table is estimated on the full sample of control and treated cantons. Robust and clustered standard errors are reported in parentheses. All estimates are weighted by number of births in the canton-year cell. Significance at the 99%/95%/90% level is indicated with \*\*\*/\*\*/\* respectively. We report the coefficient  $\alpha$  on the treatment dummy/intensity  $D_{ct}$  of Equation (3.1) where we control for time-varying canton-level characteristics, year and canton fixed effects, linear and quadratic canton-specific time trends and additionally for  $D_{c(t+1)}$ , which we also report in this Table. In the intensity specifications, we include the value divided by 100 in 2015 Swiss francs.



# Curriculum Vitae

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in Zurich, Switzerland  
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## EDUCATION

- |             |   |
|-------------|---|
| 2014 - 2019 | Fast-track Doctoral program at the <i>Zurich Graduate School of Economics</i> , University of Zurich, Switzerland |
| 2018        | Invited research stay at the London School of Economics, United Kingdom   |
| 2013 - 2016 | Master of Science in <i>Economics</i> at the University of Zurich, Part of Fast-track Doctoral program            |
| 2008 - 2012 | Bachelor of Arts in <i>Economics</i> at the University of Zurich  |
| 2011        | Exchange semester at the Humboldt University Berlin, Germany: Erasmus   |
| 2002 - 2008 | Academic high school (Matura) at the Kantonsschule Hohe Promenade, Zurich   |

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## PROFESSIONAL EXPERIENCE

- |             |   |
|-------------|---|
| 2011 - 2016 | Research and Teaching Assistant at the University of Zurich |
| 2012 - 2013 | Intern, Inflation Forecasting, Swiss National Bank, Zurich  |
| 2012        | Intern, Economic Research, UBS Investment Bank, Zurich      |

*July 2019*